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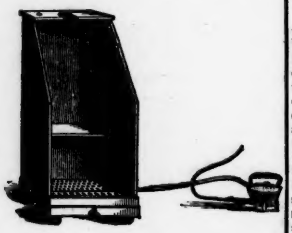

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THE

JOURNAL OF SCIENCE.

JANUARY, 1885.

I. THE MISAPPREHENSIONS OF LE VERRIER AND ADAMS AS EVIDENCE FOR THE EXISTENCE AND POSITION OF TWO PLANETS BEYOND NEPTUNE.

By the Count O. REICHENBACH.

"PENDING Problems in Astronomy" was the Valedictory Address, on September 5th, 1884, of Prof. A. C. Young, past President of the American Association for the Advancement of Science. There is more than one passage in this interesting paper about which I would like to say a word, but I confine myself to one subject.

Mr. Young says "When it is not" "unlikely that some day the searches for asteroids may be rewarded by the discovery of some great world, as yet unknown, slowly moving in the outer desolation beyond the remotest of the present planetary family;" when "some configuration in certain cometary orbits, and some almost evanescent peculiarities in Neptune's motions have been thought to point to the existence of such a world, and there is no evidence nor even a presumption against it;" and when "there is special reason for attempts to determine the rotation periods of the planets, in the fact that there is very possibly some connection between the periods, on the one hand, and, on the other, on the planets' distances from the Sun, their diameters, and

masses. (More than thirty years ago Prof. Kirkwood supposed that he had discovered the relation in the analogy which bears his name. The materials for testing and establishing it were then, however, insufficient, and still remain so, leaving too many of the data uncertain and arbitrary). Could such relation be discovered, it could hardly fail to have a most important significance with respect to theories of the origin and development of the solar system." . . .

I do not know of the "analogy," but have seen in the "Observatory" for 1880 that a Mr. Kirkwood writes—"If Laplace's hypothesis cannot be sustained, we may conclude that each planet, at its origin, was separated from a very limited arc of the equatorial protuberance, or, in other words, that instead of the separation of a ring the centrifugal force produces a rupture at the point of least resistance in the equatorial belt." Having in 1843* given reasons for the insufficiency of the original form, pure and simple, of Laplace's hypothesis and theory, I published in 1857† a theory of the common origin, development, and present organisation of the solar system, and therefore feel interested in Mr. Kirkwood's uncertain steps in the direction of a trodden path.

This theory asserted the existence of two more great planets beyond Neptune, for which I got further evidence in 1875.‡ Mr. D. P. Todd did therefore not stand alone with his meritorious inquiry into "some evanescent perturbations in Neptune's motions.§ The peremptory assertions of Prof. George Forbes‖ are too cometary to be of account.¶

It is most satisfactory that Prof. Young is not of the opinion of Prof. Newcomb. "It is true that many ingenious people employ themselves from time to time in working out numerical relations between the distances of the planets, their masses, their times of rotation, and so on, and will probably continue to do so, because the number of such relations which can be made to come somewhere near the exact numbers is very great. This, however, does not indicate any law of Nature." Does the accomplished astronomer

* Weltgebaeude. Charlottenburg, 1843, bei Bauer; and Frankfurt, 1879, bei Ruettgen.

† Gedanken eines Nicht-Gelehrten. Philadelphia, 1857, bei Thomas, pages 113 and following being translated as "Birth and Evolution of the Solar System" in "Journal of Science," London, 1883, February to April.

‡ Two Planets beyond Neptune, and the Own Motion of the Solar System. London, 1875, Wertheimer and Lea.

§ Silliman's American Journal of Science, 1881.

‖ Observatory. London, 1880, June.

¶ "Astronomical Notes," Journal of Science. London, 1881, November.

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not know that every law has its opposite and unknown? may be therefore deduced from its apparent exceptions? But perhaps he has changed his mind. Besides, his remark does not reach my theory, which gives reasons, and embraces the unknown within the origin and history of the known.

When Prof. Pierce taunted Le Verrier that the planet found by Galle was not the predicted, because all elements, longitude excepted, were wrong, Le Verrier is said to have pleaded that a more distant planet having produced errors from 5″ to 6″ might have led to these mistakes. If this reply should have been made, and is not a wrong version of a passage from Le Verrier's memoirs, we may ask why then he did not enquire more closely into the cause of the mistakes? I shall give quotations from Le Verrier's four memoirs, read before the Paris Academy of Sciences, and published in the "Connaissance des Temps" for 1849, and shall number them for reference.

"*On the Existence of a Trans-Uranian Planet.*"

1. "In the course of the year 1845 Arago represented to me that the importance of this question imposed on every astronomer the duty to concur in clearing it up to the best of his ability."

2. "The theory of Uranus has already given occasion for many hypotheses, but without scientific value, because not founded on rigorous calculations." "Everyone followed the leaning of his imagination, without furnishing reasons for his assertions."

3. Proud of his achievement Le Verrier exclaims on October 5th, 1846:—"So the position had been foreseen to less than a degree." "Thinking of the smallness of the disturbances from which the position of the star has been inferred, this success lets us hope that after thirty or forty years of observation of the new planet it may lead, on its part, to the discovery of the next in distance from the Sun, and so on."

When concurrent signs make it probable that not alone Trans-Neptunian planets, but also a lawful organisation and a limit of our planetary system exist, should an Arago not consider it "the duty of every astronomer to contribute to the utmost of his ability" towards their discovery? Because the errors of Le Verrier are best evidence for the existence and elements of these unknown, I shall point out their extent and bearing.

Admiring the mathematical powers and versatility, and the perseverant industry of Le Verrier, we must yet admit that Pierce was right, and that Le Verrier failed to discover that where he was intent on finding *one* planet, *three* made themselves felt.

4. "In 1820 we possessed forty years of regular meridian observations of Uranus. The planet had besides been observed nineteen times from 1690 to 1771." Bouvard, the author of the first Tables of Uranus, found the discrepancies in its motion so great "that he doubted if they resulted from the inaccuracy of the older observations or from some foreign not perceived action on the planet." Le Verrier sees "the only explanation of the differences between theory and observation" in the existenc "of a planet beyond Uranus."

5. Examination of all observations shows that "the period between 1781 and 1820 offers no trace of great perturbations; on the other hand, it can be connected neither with the preceding nor with the following observations."

"I said already that we satisfy well enough the movement of the planet between 1781 and 1820 without the aid of any extraordinary force. This remark, which seems to prove that the perturbing force has not exercised any perceptible influence during this period, would be sufficiently conforming to the actual hypothesis of a sudden alteration of the motion of the planet. But the period of 1781 to 1820 might connect itself naturally, be it with the former, be it with the later series of observations, and would be incompatible with only one of them. But this is not the case; it may be proved that the intermediate series cannot be made to agree on one side with the old observations, and on the other with the new ones."

Le Verrier returns over and over again to this difficulty without removing it. He calls a' half the great axis of the orbit of the *Predicted*, n' the mean motion, c' the excentricity, w' the perihelion, e' the value of the epoch year 1800, and m' the mass.

6. The first step is a guess at a'. "I said that the planet required cannot be at very little distance from Uranus, and it is not possible to place it at a very great one; for instance, at three times that of Uranus from the Sun." Le Verrier, not of the disposition of Prof. Newcomb, appeals to that metaphysical conjecture the Bode-Titius series. "Is it possible that the inequalities of Uranus are due to the action of a planet situated in the ecliptic at a mean distance double that of Uranus? And if so, where is this

planet at present? what is its mass? what are the elements of its orbit?

7. "If we might determine, at every spot, the variation of the disturbance due to the unknown mass, we might therefrom deduce the direction in which Uranus falls by the incessant action of the troubling body, and we would know the position of the latter. But the problem does not present itself in so simple a form. The numerical expressions of the perturbations might only be immediately concluded from the observations, if we rigorously knew the elements of the ellipsis described round the Sun by Uranus; these elements are again not to be exactly determined unless we know the quantity of each of the figures of a theory. They allow to treat different points of a question without an error with regard to one of these influencing the other. However, it does not appear to me that we might follow this road with perfect safety for the whole work, when we do not know a single relation to be satisfied by the result obtained, and which might serve for their verification. The mutual dependence causes that if the work is not completely correct it is necessarily wrong in every point. Or we comprehend that it is easier to escape this alternative than to the manifold chances of an isolated error."

"Indeed after having treated all perturbations simultaneously, let us return to the calculation of a single one by a direct method; its verification will require that of the whole work. But if, instead of thus controlling a single inequality, we determine successively each one by direct calculation, and then the new results coincide with the first one, all kind of error becomes impossible. It is this double work which I consider necessary to impose on myself in this case."

From this it appears that, notwithstanding this double work, when the disturbances observed and calculated—not knowing "a single relation to be satisfied by the results obtained"—are ascribed to one mass instead of three, the verification of the whole work which is all error will be required, turning the negative or evidently false results into new hypotheses of the existence of one, two, or more masses, as the case may indicate, utilising the one result confirmed by observation, as either accidentally right or as indicating an organic law. This is what I shall endeavour to do, as far as ability and time permit.

8. The following, from the final memoir, is indeed a return from the second to the first method with its dangers. "The methods which, like the preceding, lead at once to determine the perturbations separately one from the other, are most

valuable. But we see the impossibility to separate into distinct parts the research of the elements of Uranus, and of the elements of the disturbing body. It would be vain to hope that in forming empirical equations we might discover, *à priori*, the law of disturbance, but we should run the risk to grossly deceive ourselves, because we should only have obtained an expression qualified to represent the excesses of the disturbances over the errors arising from the inexactitude of the elliptical elements, but not at all the disturbances themselves. There is only one way; it is to form the expressions of the disturbances caused by the new body into functions of its mass, and of the elements of the ellipsis it describes: these disturbances have to be introduced into the co-ordinates of Uranus, calculated by means of the unknown elements of the ellipsis this planet describes round the Sun. Equalising these co-ordinates so obtained to those observed, we take for unknown in the equations of condition which thereby result not only the elements of the ellipsis described by Uranus, but also the elements of the ellipsis described by the disturbing planet of which we want the position."

But when the ellipticity and half the great axis of the orbit and the mass, and everything attributed to the unknown by Le Verrier, differ so much from those of Neptune, how could he be found in the longitude of the predicted?

The answer shall prove the existence and give the approximate longitude, and more or less the other elements of the two planets A and B.

"The observations extend only over 155 years." The 155 must be embraced in a second solution, "regardless of the amount of labour," after 78 years proved too little.

9. "We conclude that ε' falls between 94° 40′ and 189° 55′, or between 263° 8′ and 358° 71′, to make the value of m' positive.

10. "We so might get a negative value of m' corresponding to the positive value of ε', whereby the true solution of the problem would be misapprehended. I freely avow that this is what first happened to me, and that the difficulty long detained me."

11. "We see that the value of m' actually changes sign for values of ε' included between the points of the circumference obtained."

12. "To definitively know if there exists in this extent a convenient value of ε', we had now to attribute different arbitrary quantities to this variable, calculate the corresponding values of m', $m'h'$, and $m'l'$, and examine if one of the

solutions so obtained may represent the motions of Uranus in 1690 and 1747."

13. "When we exclude the negative values of m', and the positive ones too considerable not to be rejected, according to reasons particular to the motions of Saturn, we have to admit that the value of ϵ' suiting the problem, or examining the errors of the theory, leaves it in the position of the planet in 1690 to 1747 for all the values of ϵ' between the limits just indicated, we see that the calculations always differ from observation for at least one of these positions,—a quantity much too great to be attributed to an error of observation, so that the consequence resulting from the discussion seems to me the impossibility to represent the march of Uranus by means of the perturbing action of the new planet."

14. Le Verrier arrives at a contraction of the more or less opposite arcs; "we have to admit that the value of ϵ' suiting the problem must be comprised between 108° and 162°, or between 297° and 333°," but "calculation always differs from observation for one at least of these two positions," and we again arrive at the "impossibility to represent the march of Uranus by the disturbing influence of the new planet."

15. "Let us remark" "that the conclusions at which we would arrive may not be the expression of the truth. The motives given, to legitimate this way of judging, will receive new force by the following remark. Imagine that we went through the preceding discussion omitting the two following inequalities in longitude." (— —.) "These two disturbances are so feeble that it seems they might be omitted without affecting the final result. But nothing of this."

16. "The preceding details seemed indispensable to make the reader understand all the difficulties in questions like that before us. Without them we could not have seized the importance of the modification made in the preceding march to definitively solve the problem." "The result to be arrived at will confirm in a remarkable manner the previsions of No. 8. We shall prove, contrary to the conclusions which seemed to follow from the preceding discussion, that it is possible to represent the march of Uranus, in accounting for the disturbing influence of the new planet; that the length of the epoch ϵ' of this new star must be 240°; and that this hypothesis, the expression of the truth, would be repulsed by the negative value found for the mass."

17. "It is sufficient to discuss in the same manner the error of the theory in 1758, and, in attributing increasing

values to e', to become aware that they are all inadmissible from 99° inclusive. *From beyond that point towards* 108° it would not be impossible to find admissible values for m', P', Q', neutralising the error of theory in 1758 or 1747, when we consider *one* of the epochs *by itself*."

"We perceive, however, that the errors of the theory incessantly go on diminishing as e' increases from 189° forward, and that the errors become simultaneously very small when e' attains 245° to 252°." Beyond new change on to 360°.

18. "It appears, therefore, that we can effectually account for the motions of Uranus by means of a disturbing planet."

19. "We may infer that the mean longitude of that planet was 243° to 252° on 1st January, 1800."

Le Verrier finally assigns to the Predicted a h.c. longitude on the 1st of January, 1847, 326° 32′; a mean solar distance, 36·156; an excentricity of orbit, 0·1076; a longitude of perihelion, 284° 5′ 48″; an inclination of orbit to that of Uranus of at least 4·38; a precession from 1st of January, 1800, in 47 years 0′39·20; a period 217·387 years, and a mass 1÷9322 of the Sun.

20 "The new planet whose existence and place was fixed in heaven by the investigation just read" was found by Galle at Berlin, two days after he had received the data from Le Verrier.

The h.c. longitude got by observation was for January 1st, 1847	327° 24′
That assigned by Le Verrier	326° 32′
Difference...	0° 52′

When I venture, from a metaphysical standpoint, to criticise this scientific triumph, it is not to disparage the merits of the accomplished analyst, but to prove by his mistakes that the organisation of the solar system and the two planets exist.

From the publications quoted it may be seen when and why I made the planetary mass 1÷680 of the solar, divided it in 680 parts, and assigned to Uranus, Neptune A and B 104·2 parts, and to the two latter the other elements as given in the Table; and the h.c. longitude, January 1st, 1875, to A 196° with error in more, to B 308° with error in less.

Biassed from my first view by the then adopted masses, I distributed the 1÷4468 of the solar mass:—Uranus, 1÷24608; Neptune, 1÷14446; A, 1÷12962; B, 1÷22927. I now accept Prof. Newcomb's 1÷22608 for Uranus, 1÷19360 for Neptune, together 1÷10428, and divide the remaining 1÷7726 according to the "development theory"—

$$A : B = 1 : 1 \div (1{\cdot}22^3 \times 1{\cdot}025^3) = 1 \div 13200 : 1 \div 18619.$$

Uranus + Neptune to A + B, as Mercury + Venus to Earth + Mars; moons, asteroids, &c., inclusive.

That the existence of the two planets and their elements, longitude excepted, were determined by the metaphysical development theory, before I knew at all of Le Verrier's misapprehensions, is certainly evidence for the theory.

Le Verrier first found a positive m', and therewith e', to fall between 94° 40′ and 189° 55′, or between 263° 8′ and 358° 41; then in narrower limits, between 108° and 162°, or 297° and 333°; there were two chances to select from. But "for certain reasons" both are "inadmissible;" we get to an "impossibility" against the nature "of the problem." "There is only one way" to its "final solution;" it is to form the expressions of the disturbances caused by the new body into functions of its mass and of the elements it describes. A negative m' is metamorphosed into a positive "between 245° and 250°." "From beyond 99° inclusive towards 108° it would, however, not be impossible to find admissible values," "when we consider *one* of the epochs *by itself*."

Neptune is, of the outside planets, the one most perturbing Uranus, and the greatest perturbations must fall in the time of conjunction. Such proximity of the planets was therefore indicated; it fell within the time of observations during the 155 years. Le Verrier therefore endeavoured to bring his *one* planet, to predict, at the right time in this longitude. But as Uranus was also disturbed by A and B he had to invent, for his *one* planet, elements suiting his wants.

He concluded a compromise: he cast Neptune and A into one. He invented a solar distance not that of Neptune, and a mass not that of Neptune plus A, for a fictitious being, and more or less reconciled the discrepancies between theory and observation in the distribution of the individual disturbances in magnitude and time by a great excentricity, 0·1076, that of Neptune being 0·0089; and a perihelion in the conjunction longitude of Uranus and Neptune 284° 45′,

that of Neptune being in 46° 6′ 39″. He placed his unknown almost in the middle between Neptune and A, at 6·120 from the former at 5·844 from the latter. According to the excentricity intercalated, between Neptune and A, for the Predicted, it moved 3·89 farther, and nearer than its mean distance from Uranus. Le Verrier placed the mean orbit of the Predicted at 16·97 from that of Uranus: Neptune is at 10·85. This reduces the perturbing power in its mean, comparing Neptune to the Predicted,—

$$16{\cdot}97^3 \div 10{\cdot}85^3 = 2{\cdot}46 \text{ times.}$$

This loss of power by distance had to be balanced by mass.

21. "The search for the value of m' is a very delicate point," says Le Verrier. The mass of Neptune is $1 \div 19360$; Le Verrier made that of the Predicted $1 \div 9322$, about $1 \div 26$ more than double, but not that of Neptune plus A. Had he done this he would have balanced loss by distance by mass, for $1 \div 13200 + 1 \div 19360 = 1 \div 7848$ exceeds the mass of Neptune 2·46 times. As it was, he attributed to his planet only a mass 2·077 times that of Neptune, and left against the loss of power by distance an unsatisfied balance of 0·38.

When we apply the distance and mass attributed to the Predicted, we find that it would exercise at its perihelion an influence 1·423 times greater, and at aphelion 1·789 times less, than that exercised by Neptune at mean distance, which is in sum a variation of $1 \div 3{\cdot}212$ in the disturbing power of the Predicted.

When we apply the elements of Neptune and A in comparison, the former exceeds A in perturbing power by distance $2{\cdot}103^3 = 4{\cdot}42$ times, and acts less by mass 1·46 times. Neptune therefore exceeds A in this power 3·027 times, which gives a difference of only 0·185 with the variation 3·212 of power by excentricity of the fictitious planet.

For such slight differences various causes may exist; but the dominant cause is B, whose perturbing power is $1 \div 26$ of that of Neptune and $1 \div 8{\cdot}7$ of that of A. It moved in 155 years only through about 88°. When B came in conjunction with Uranus and Neptune it became a part substitute for far-off A: the excess of 0·077 over the 2, in the point of mass, may be therefore considered as equivalent of the $1 \div 26$, and the deficiency 0·38 of mass balancing distance as more or less representing the absence of A in the time of conjunction of the other three planets.

The disturbance of Uranus by B may be indeed traced as

concrete and dissolved in the most "rigorous calculations," —a remark illustrated by the following quotations.

22. "All these" minor "differences between theory and observation may be very well put on account of the latter," or may be accounted for by B.

23. "There will be no exception" to the perfect agreement of theory and observation, "but for equation (25) founded on the observations from 1824 to 1827, the error, which remains almost always the same in the equation, whatsoever be the hypothesis, is from 5″ to 6″."

Le Verrier abandoned for the *one* planet the sound basis of his most "rigorous calculations," where in 1800 the two arcs with positive m' included the first one A, the second B, and had Neptune in the midst of their neighbouring terminals $(189° 55' + 263° 8') \div 2 = 226° 31'$. He struck out for the periods and longitudes of greatest disturbances as those of probable conjunction of Uranus and the one planet, connected these periods by an ingenious combination, and obtained a success.

The Predicted was not Neptune; but Le Verrier has not only indicated the longitude of one planet out of three, but also that of A and B.

		Uranus.	Neptune.	Predicted.	A.	B.
Solar distance..	—	19·18	30·03	36·156	42	76
Perihelion	—	167° 32′	46° 6′ 39″	284·45°	9°?	62°?
Excentricity	—	0·0046	0·0089	0·1076	0·0041?	0·1156?
Revolution years	—	84·07	166·6	217	272	662
Conjunction years	—	—	171·4	136·6	121·34	97·3

Longitude on January 1st:—	Year.					
	1778	79·3°	—	—	79·736	—
ϵ'	1800	174°	224·5°	—	108·776°	279·573
Motion taken	1825	280·84°	279·308°	—	141·766°	279·573
Circular for A and B ..	1847	—	327° 24′	326° 32′	—	—
	1885	—	—	—	221° or less.	314° or more or less.
	1899	239°	—	—	239·256°	—

The greatest differences observed during the 155 years, indicating positive m', fell in the period between 1824 to 1827, and in 1690. After two failures to satisfy all observations by the *one* planet, Le Verrier put the perihelion of an orbit into 284° 45′, about the longitude of Uranus, on January 1st, 1826. The time from 1690 to 1826 is 136 years, the

mean period of conjunction of Uranus and the Predicted or fictitious; but, as the latter was not Neptune, it was already slightly behind the real planet in 1847, and had been in advance in 1800.

I now refer to quotations 9 and 17. "From beyond that point towards 108·776°," &c. I select 108·776 (s. quot. 14) to place it against the year 1800 for A. There was a conjunction of A with Uranus in 1778, between the observations of 1771 and 1780. I placed slow-moving B in 1825 at 279·573°, before I saw Le Verrier's memoirs; circular motion then gives 265·348° in 1800. We had in the "second solution" 108° as beginning of an arc with positive m'; we have in the first solution 263° 8′ as beginning of the second arc. Neptune, as before said, was right in the middle of the negative m' between an ending and commencing arc with positive m', with A and B between 189° 55′ and 263° 8′ in 226° 32′.

Going back with conjunctions with Uranus before 1670, we meet them with B about 1630; with Neptune about 1654, with A about 1657, in such quick succession that they throw their combining shadow before them to 1690.

Of Prof. Adams's investigation I know only what Sir G. B. Airey wrote in the "Monthly Notices of the Royal Astronomical Society" (1846). His first hypothesis had distance 38·4 based on the Bode-Titius series, the second "about 1-30th less." He adopted—if from the commencement or not does not appear—the method Le Verrier followed in his "final solution." His figures were—first and second hypothesis distance, 38·4, 37·12; perihelion, 313° 57′, 299° 11′; excentricity, 0·16103, 0·120615; mass, 1÷6040, 1÷6666; longitude (Oct. 1st, 1846), 325° 8′, 323° 2′.

No. 1 placed the aphelion at distance 42·1; No. 2 at 41·59. The mass for No. 2 is 2·904 times that of Neptune; the change of power on Uranus from perihelion to aphelion is 2·77 to 1. The loss of power from Neptune distance to aphelion distance of the fictitious is 4·26 to 1. We perceive here the difference in the treatment of mass by Le Verrier and Adams. The masses A to Neptune we had 1·46 to 1 and $4·26 \div 1·46 = 2·91$.

It is easily seen that the first hypothesis—aphelion 42·1, the distance of A, giving the mass 3·205 times that of Neptune—reproduces the relation of power on Uranus from Neptune and from A by the series of fictions invented to find *one* planet instead of two, and that the second hypothesis originated from the interfering B, the third planet, to remove the error of from 5″ to 6″ mentioned by Le Verrier.

We can only wish that the distinguished mathematician would once more apply his talents and influence to secure the early theoretical and visual discovery of the two still unknown great planets of the solar system.

II. RECENT DEVELOPMENTS OF CHEMICAL SCIENCE.

SEVEN years ago we ventured to insert in the "Journal of Science" a paper bearing the somewhat ambitious title "The Chemistry of the Future." In that paper emphasis was laid on the unsatisfactory state of our knowledge concerning the simple bodies or so-called "elements," and attention was called to certain lines of research which promised to throw further light upon this difficult though interesting question. Of these investigations two seemed to offer direct evidence that the elements are not primordially distinct bodies, but compounds of a smaller number of elements of a higher order, or perhaps have even been formed by the "expansion"* of one aboriginal principle. We refer here, in the first place, to the researches of Prof. Victor Meyer on the halogens—chlorine, bromine, and iodine. The phenomena observed by this chemist seemed to admit of the conclusion that, given a sufficiently high temperature, bromine and iodine might be resolved into chlorine. But this prospect has since faded away. It is generally admitted that Prof. V. Meyer's experiments afford no conclusive proof that either bromine or iodine has undergone incipient decomposition. It is needless to add that in such a matter, where the temptation to indulge in hypothesis is naturally so strong, nothing short of absolute demonstration can be accepted as satisfactory.

The second line of investigation, which it was hoped might supply evidence of the decomposition of some of our ordinary elements, has fared little better. The spectroscopic researches on the constitution of the sun and certain of the fixed stars—with which the names of Mr. Lockyer and Dr.

* *See* Journal of Science for January, 1877, p. 26.

Huggins are especially connected—seemed to show the behaviour of certain elementary bodies at temperatures prodigiously higher than any which we can produce in our laboratories. But here, again, though the investigation has not been brought to a definite conclusion, the verdict of the most judicious chemists and physicists is, so far, "Not proven." Prof. Sir H. E. Roscoe, in his opening address at the meeting of the Chemical Section of the British Association at Montreal, does not refer to the decomposition of any of the elements as a subject within the present scope of practical research.

In proof of the difficulty of deciding such a question by spectroscopic research alone, Prof. Liveing stated at the Montreal meeting that cadmium, mercury, and zinc, as well as aluminium and tin, give no spectra at all at sufficiently high temperatures. Iron, on the other hand, shows so many lines that it either must have a very complex constitution or else it is really formed of a number of elements having nearly identical atomic weights, and which, so far, defy separation.

We are therefore driven back to what may be called the indirect evidence for the compound nature of the elements. Under this head we gave, in 1877, an exposition of the "periodic theory" of Mendelejeff and Lothar Meyer, or rather, we should say, of Newlands. The natural relations of the elements thus brought to light are scarcely conceivable if these bodies are initially distinct, independent entities, but are quite compatible with the supposition that they have been evolved from some common principle.

Now the evidence in favour of the "periodic law" has been of late materially strengthened. It is more and more found to lend itself to the rational interpretation of new classes of phenomena. One of its most interesting applications has recently been made by Prof. T. Carnelly to the state in which the elements occur in Nature. This chemist refers, in the outset of his memoir ("Berichte der Deutschen Chemischen Gesellschaft," vol. xvii., p. 2287), to three facts laid down by Mendelejeff in his first paper on the Periodic Law ("Annalen der Chemie und Pharmacie," Supplement 8). These three facts are—

a. That although all elements belonging to the same group evince close natural relations, yet the elements of the even series are more nearly connected with each other than with those of the odd series, whilst in like manner the elements of the odd series are

more nearly related to each other than to those of the even series.

b. In the elements of the second series the foregoing rule is often reversed from carbon to fluorine, and in the third series from sodium to silicon inclusively. This appears very distinctly on examining Lothar Meyer's "curve of the elements." See his "Moderne Chemische Theorien," or Pattison Muir's "Principles of Chemistry," p. 227). This curve, on reaching carbon, instead of falling, rises until it meets sodium, and then falls from sodium to silicon, instead of rising. This inverted character of the curve accurately illustrates certain properties of the elements which lie within the limits above mentioned.

c. The elements belonging to Mendelejeff's eighth group are peculiar in many respects. They form, unquestionably, the connecting-links between the even series on the one hand, and the odd series on the other. This fact is illustrated by Meyer's curve, which shows that they form, step by step, a transit from the even to the odd series, or rather from the ascending to the descending parts of the curve. The elements of this group are found at the lowest points of the curve.

After having thus laid down the preliminaries upon which he takes his stand, Prof. Carnelley proposes the following law:—

1. Elements which belong to uneven series are, as a rule, easily obtained in the free or uncombined state, whilst those belonging to the even series cannot without difficulty be liberated from their combinations.

The only exceptions to this rule—somewhat numerous, it must be confessed—are carbon, nitrogen, oxygen, sodium, aluminium, and silicon, the explanation of which is found under Mendelejeff's rule *b*, and also the elements of group VIII. (See *c*.)

2. As regards the occurrence of the elements in Nature, in a free state, a point naturally connected with their readiness in being expelled from combination we have then the following law:—

Elements belonging to the even series (with the exception of carbon, nitrogen, oxygen, and the eighth group) never occur in Nature in a free condition, whilst the elements pertaining to the odd series may generally speaking, and even frequently, be found in the free condition. Instances of

simple bodies thus frequently met with uncombined are—Copper, silver, gold, mercury, arsenic, antimony, bismuth, sulphur, selenium, and tellurium; lead and tin occasionally occur naturally in the metallic state. Gallium, indium, thallium, and cadmium are of such rare occurrence that we can scarcely venture to state whether they occur in the free condition or not. Among the bodies which belong to the uneven series, and still are not met with in the free state, only chlorine, bromine, iodine, fluorine, zinc, and phosphorus are noteworthy exceptions, whilst sodium, magnesium, aluminium, and silicon are explained by Mendelejeff's principle *b*. As regards the exceptions among the even series, carbon, nitrogen, and oxygen come under *b*, and the group VIII. under *c*.

3. As concerns the occurrence of the elements in Nature in a combined condition, Prof. Carnelley, leaving the elements chlorine, bromine, iodine, and fluorine, and the group VIII., for future consideration, proposes that—

The elements belonging to the odd series generally occur in Nature as sulphides or double sulphides (or selenides, tellurides, and arsenides),—*i.e.*, in combination with a negative element belonging to an odd series,—and only in very rare cases as oxides. Elements belonging to the even series are generally found as oxides or double oxides (with the formation of silicates, carbonates, sulphates, aluminates, &c.),—that is to say, with a negative element belonging to an even series,—and in two instances only as sulphides.

Thus the following elements belonging to the even series occur as oxides or double oxides:—

Commonly: Lithium (in lepidolite, &c.), potassium (in saltpetre and felspar), rubidium, cæsium, beryllium, calcium, strontium, barium, boron, scandium, yttrium, lanthanum, ytterbium, carbon, titanium, zirconium, cerium, thorium, vanadium, niobium, didymium, tantalum, oxygen, chromium, tungsten, manganese.

Frequently: Nitrogen (in saltpetre), molybdenum.

Rarely or never: None.

As sulphides:—

Commonly: Molybdenum.

Very rarely: Manganese, also oxygen as sulphurous acid in volcanic gases.

Elements belonging to uneven series occur as sulphides (selenides or tellurides):—

Commonly: Copper, silver, zinc, cadmium, mercury,

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gallium, indium, thallium, lead, antimony, sulphur, selenium, and tellurium.

Frequently: Arsenic, bismuth, tin.

Never: Gold (occurs only in the free state), sodium, magnesium, aluminium, silicon, phosphorus. (See *b*.)

As oxides:—

Commonly: Sodium, magnesium, aluminium, silicon, phosphorus (see *b*), and tin.

Frequently: Zinc and copper.

Rarely: Lead, antimony, bismuth, and arsenic.

Group VIII.—The elements of this group, with the exception of iron, cobalt, and nickel, never occur in combination. Hence we need consider merely these three bodies.

Iron occurs mostly as oxide, but still also frequently as sulphide. Cobalt is met with mostly as sulphide, but often also as oxide. Nickel occurs chiefly as sulphide and arsenide, and but very rarely as oxide.

Iron, the first member of this group, approaches closely to the elements of the even series, whilst nickel, the last member, approximates much to those of the odd series. This is a proof that group VIII. affords a gradual transition from the even to the odd series.

The halogens, chlorine, bromine, iodine, and fluorine—the most electro-negative elements—occur in Nature in combination with the most electro-positive metals, as chlorides, bromides, iodides, and fluorides, and (if we disregard a few metallic oxychlorides and sulpho-chlorides) never in combination with oxygen or sulphur.

The above-mentioned facts may thus be summarised with reference to Lothar Meyer's elemental curve:—

Elements whose position is on descending parts of the curve are reduced with difficulty, and never occur in Nature in the free state or in combination as sulphides, but always in union with oxygen as oxides or double oxides (silicates, sulphates, carbonates, &c.); whilst the elements which have their position on ascending points of the curve are easily reduced, and occur almost invariably to a greater or less extent in a free condition, or in combination with sulphur, and very seldom with oxygen.

Another investigation by Prof. Carnelley likewise deserves notice here, as it connects itself with the periodic law, and thus, indirectly at least, strengthens the evidence in favour of the compound nature of our supposed elements.

The more immediate object of the author's researches is

the colour of chemical compounds, viewed as a function of the atomic weights of their component elements. In a memoir on this subject communicated to the "Berichte der Deutsch. Chem. Gesell." (vol. xvii., p. 2151), he remarks that the colour of chemical compounds depends on at least three conditions, namely,—1, the temperature; 2, the quantity of the electro-negative element; and 3, on the atomic weights.

The two former of these conditions have been especially studied by Ackroyd ("Chemical News," xxxiv., p. 76). Long previously a paper on the second of these conditions was read before the Chemical Section of the Manchester Philosophical Society. This took place between the autumn of 1849 and that of 1853; but as we cannot give the precise date, and have no means of referring to the original document, we suppress the name of the author. His conclusions agreed very closely with those reached by Mr. Ackroyd under the second head.

The conclusions of the latter chemist are—

I. All the chrome compounds change their colour in a fixed succession, in the order of the spectral colours, and in such a manner that the colour, as the temperature rises, approximates more and more to the red end of the spectrum, and passes ultimately into brown and black if the heat is sufficient. The transition of colour is generally direct from white to a pale yellow, violet, indigo,—blue and green being overleapt.

II. In binary compounds an increase of the proportion of the electro-negative element involves a modification of colour towards the red end of the spectrum, and finally into brown and black. Thus PbO is yellow, Pb_3O_4 red, and PbO_2 brown.

III. Prof. Carnelley's law as to the influence of the atomic weight is as follows:—In certain series of compounds, A*x* R*y*, B*x* R*y*, C*x* R*y*, &c., in which R is an element or group of elements, whilst A, B, C., &c., represent elements belonging to the same sub-group of the periodic classification, the colour passes either entirely or partially through the following scale:—White or colourless, violet, indigo, blue, green, yellow, orange, red, brown, black,—or, in other words, the higher the atomic weight of the elements A, B, C, &c., the more the colour of the compound approaches the red end of the spectrum, passing in certain cases into brown and black.

It must be observed that the above law refers only to cases in which A, B, C, &c., are elements belonging to the same sub-group, but not to cases where they belong to different sub-groups. Thus oxides cannot be directly compared with the corresponding sulphides, selenides, and tellurides. Nor can fluorides be compared with the corresponding chlorides, bromides, and iodides, for fluorine and oxygen belong to even series, whilst chlorine, bromine, iodine, and sulphur, selenium, and tellurium pertain to odd ones.

It must likewise be remembered that the above rule applies strictly only to solid compounds, and that in the solid state the colour can within certain limits change with the condition of aggregation.

In addition to 426 cases in which the law has been verified, Prof. Carnelley meets with fourteen exceptions. Four of these, $DiCl_3$, V_2O_5, CrO_3, and CaO, do not seem to admit of any satisfactory explanation. He considers that the colour of a compound will ultimately prove to be a periodic function of its atomic weight. This may best be seen on considering the normal iodides. If we construct a curve in which the ordinates represent the atomic weights of the positive elements, and the abscissæ form a scale rising from black, through brown, red, orange, yellow, green, &c., to white, the curve thus obtained resembles in figure Lothar Meyer's well-known elemental curve.

Could Prout's hypothesis—*i.e.*, that the atomic weights of all the elements are multiples by whole numbers of that of hydrogen (=1)—be demonstrated, it would have contributed no little to prove that these bodies are genetically connected. But the most careful re-determinations of the atomic weights lead us in the very opposite direction. Instead of the exceptions to the supposed law disappearing, on more exact scrutiny they persist, or are even increased.

One indirect consideration in favour of a common evolutionary origin of the elements has, perhaps, scarcely received the attention which it deserves,—*i. e.*, the occurrence in intimate admixture of bodies having very similar properties, and sometimes even closely approximating atomic weights. Such groups are, *e.g.*, cobalt and nickel, the three halogens, and especially the rare earths found together in samarskite, &c. It is difficult to frame a hypothesis which will account for the co-existence of these various groups of bodies, save the one that they have been evolved out of some common material.

But this, and all other indirect evidence pointing in the

same direction, is very far from meeting the demands of Science, We are no nearer than we were half a century ago to the decomposition of any element, or to the metamorphosis of one into another. In one sense even we are further off. Considering the greatly improved means of research which chemists now have at their disposal, and the number of well qualified *savants* who have attacked this problem from different sides, we feel that the probability of a solution is lessened. We can only hope that some untried path, of which the world has no conception, may some day lead to the desired end.

III. NEW RESEARCHES ON THE FECUNDATION OF ANIMALS.

FECUNDATION AND SUBSTITUTION.

THE remarkable advances made recently in the study of the fecundation of animals are known to all biologists. A recent monograph by Prof. E. van Beneden, summarised in the "Naturforscher," developes a new apprehension of the essence of this process, and compels us to recur to this important question.

The modern views on the process of fecundation, as they are now universally accepted, date back to the researches of Bütschli. This *savant* was the first to show that in the yolk of the fecundated ovum of *Rhabdites dolichura* there appear two nuclei which soon move towards the centre of the yolk, and coalesce to form the nucleus of the first embryonic cell. Two years afterwards, in 1874, this discovery was confirmed by the researches of Auerbach. A little later Bütschli discovered, further, that in place of the germinal vesicle there appears a fibrillary spindle (the spindle of direction) like that which is formed in the cell-nucleus in the moment of segmentation. This spindle takes part in the formation of the polar vesicles, which, as it is known, are expelled from the ovum.

In 1875 E. van Beneden found that the observations which Bütschli had made on Nematoids hold good also as regards mammals. He observed, further, that the two nuclear elements possess different properties. The one makes its appearance near the surface of the ovum at the moment when the other already occupies the centre of the yolk. Both germs, as they approximate to each other, increase in size. Even after the union of both elements in the centre of the yolk, the nucleus which has been formed at the circumference possesses attributes which serve to identify it with the essential constituent of the spermatozoa. Hence the pronuclei may be distinguished as a male pronucleus derived from the periphery of the ovum, and a female formed in the yolk.

Soon afterwards there appeared the important investigations of O. Hertwig, in which it is proved that the sperm-nucleus is evolved from the spermatozoon, and the germ-nucleus from the yolk, which then unite in the well-known manner. Fol subsequently observed how the spermatozoon penetrates into the yolk. This fact—the penetration of the spermatozoon and its share in the formation of the male pronucleus—has been verified by all succeeding investigators. Observers differed among themselves only in the manner in which this penetration occurs, and on the part which it plays in the formation of the male pronucleus. Broadly speaking, the views of Hertwig and Fol—that the spermatozoon finds entrance into the yolk, that a portion of it becomes the sperm-nucleus which combines with the ovum-nucleus to form the segmentation-nucleus—were universally acknowledged. On the minor details, such as the formation of the fibrillar spindle and the stellar bodies or polar vesicles, their signification and destiny, the manner of the penetration of the spermatozoon and its part in the formation of the sperm-nucleus, there still prevailed differences of opinion.

This posture of affairs induced Beneden to resume the investigation of the process of fecundation. For his observations he selected *Ascaris megalocephala*, the most suitable subject at once on account of its size and the simplicity of its organisation.

In his monograph detailing the results of his investigations, after a preliminary account of the present state of the question and of the methods employed, he discusses the changes which the ovum undergoes during the first period of ripeness. He then describes the entrance of the spermatozoon into the ovum, or the conjunction of the sexual products; he considers the second period of the maturity of

the ovum, and the changes which the spermatozoon experiences in this period. He takes up next the formation of the male and the female pronucleus, the fecundation and the segmentation of the first embryonal cell. Finally he gives the conclusions deduced from the results of his observations, which we shall here summarise from the "Archives de Biologie" and the "Naturforscher":—

The ovarian ovum yields to the ovum-gonocyte a protoplasmic body (which is reduced by the absence of the matter forming the strata deposited around the yolk), and a nucleus which is reduced by the repulsion of the partly chromatic and partly achromatic elements which enter into the composition of the polar vesicles. The germinal vesicle undergoes a first reduction at the time of its apparent disappearance. A part of its substance is then mixed with the protoplasm of the egg, and this reduced nucleus is the female pronucleus.

On a former occasion I used the term gonocyt to express a cell-body which possesses two nuclear elements with different sexual properties. I have applied this name to the organism of the Infusoria which possess an endoplasm and an endoplastula, and also to the body of the ovum which possesses both pronuclei. But it follows clearly, from the study of fecundation in *Ascaris megalocephala*, that an ovum possessing its two pronuclei is exactly the equivalent of an ordinary cell. I have proved that in the nuclei of each of the two first blastomeres (segmentation-cells) we find the equivalent of a male semi-pronucleus and a female semi-pronucleus. It is also probable that the same will hold good in the cells thence derived. A cell with a single nucleus differs thus in nothing from a cell with two distinct pronuclei.

An examination of the division of a cell with two pronuclei shows that it takes place exactly like that of a single cell. A cell with two pronuclei cannot, therefore, require a distinctive name. It is useless therefore to employ the word gonocyt in order to denote this hermaphroditic element, or any other cell. But instead of banishing this word from the vocabulary of the cell-doctrine (we hesitate at saying cellology) it seems to me that it might be usefully retained in a modified acceptation. I propose to use it with the respective addition of the adjectives "female" or "male," in order to designate the ovum reduced in consequence of its ripeness, and the spermacyt after the repulsion of its cytophoral portion after it has become a spermatozoid.

But if from a morphological point of view, and considered

in their structure, the pronuclei differ in nothing from ordinary nuclei, it is clear that, physiologically speaking, they are in no wise equivalent to nuclei. Each pronucleus is rather equivalent to a half-nucleus which presents, from its origin, a uni-sexual character. This is the justification for the term "pronucleus," which I originated, and which is now generally adopted. But if the female pronucleus is not the equivalent of an ordinary nucleus it is clear that the yolk-globe, with its pronucleus, is not a cell. If a particular name for the nuclear element is justifiable, it is, in like manner, for the cell-body. I propose, therefore, the name "female gonocyt" for the yolk-globe with its pronucleus.

As for the spermatozooid it is distinguished by many peculiarities from every true cell-element. We never observe the division of a spermatozoon. Its nuclear element becomes a pronucleus, and is therefore not equivalent to a nucleus. Hence the body of the spermatozooid is not a cell, and it may be suitably known as a male gonocyt. This element is distinguished in its origin from every other cell by the circumstance that the spermacyt, from which it is derived, becomes a spermatozoid only by divesting itself of a part of its substance. The part thus thrown off by the spermatocyt is the cytophor, or more correctly the cytophoral portion.

I do not need to revert here to the view which I have advanced above concerning the signification of the phenomenon of fecundation. I am of opinion that that fecundation consists essentially in the completion of the female gonocyt and its transformation to a cell,—that is, in the replacement of the eliminated elements of new matter derived from the spermatozoon. The eliminated nuclear elements (polar vesicles) are replaced by the male pronucleus; and as the polar vesicles together with the female pronucleus formed a cell-nucleus, the germinal vesicle in like manner, the male pronucleus, and the female pronucleus form together a single cell-nucleus. It is indifferent whether these two pronuclei coalesce visibly to a single element, or if they remain distinct. There exist grounds for the assumption that even in the ordinary nuclei the male chromatine remains separate from the female chromatine.

The protoplasm of the male gonocyt is mingled likewise with the body of the female gonocyt. But is this a substitution? Is it in the same sense essential as the substitution of a part of the nucleus? The question may be discussed, but for the present it is hard to decide.

After Hensen, in his treatise on Generation, has expounded the views of Hertwig on the nature of fecundation, he adds:—"These are the views which are opening out; their full theoretical elaboration has not yet taken place, partly because the behaviour of the nuclear threads or the original nuclear substance has not yet been examined, partly because the fact that the head of the spermatozoid becomes a nucleus is still contested." On the other hand, Flemming, after showing that the chromatine of the sperm-nucleus mixes with the female pronocleus, and contributes to increase the quantity of the chromatine of this latter element, admits that he does not know what becomes of the light halo surrounding the chromatine of the sperm-nucleus:—"What becomes of the light halo which still surrounds the central corpuscles of the sperm-nucleus, when the the two nuclei meet together, I have not been able to ascertain."

I believe I have taken an additional step in the comprehension of the phenomena of fecundation by showing:—

1. That not merely the chromatic nucleus of the spermatozoon, but also the surrounding achromatic layer (perinuclear stratum, participates in the formation of the male pronucleus.
2. That the germinal vesicle furnishes the female pronucleus not only with chromatic elements, but also with an achromatic body.
3. That the two pronuclei, without becoming intermixed, may attain by progressive maturation the constitution of ordinary nuclei.
4. That in *Ascaris megalocephala* a single nucleus is not formed out of the two pronuclei, and that a segmentation-nucleus—in the sense which O. Hertwig gives to the word—does not exist. The essential point of fecundation does not lie in the union of the two nuclear elements, but in the formation of these elements in the female gonocyt. One of these nuclei is derived from the ovum, and the other from the spermatozoon. The nuclear elements expelled in the form of the polar elements are replaced by the male pronucleus, and as soon as two half nuclei (a male and a female) have been formed the process of fecundation is completed.

In consequence of a series of transformations which the nuclear structure of each pronucleus undergoes,—transformations identical with those which take place in every nucleus undergoing division,—each pronucleus produces two

chromatic loops. These four chromatic loops take part in the formation of the chromatic star, but they remain distinct. Each of them becomes divided longitudinally in two secondary twin loops.

The nuclei of both the first blastomeres receive each a half of each primary loop, or four secondary loops, of which two are male and two female.

There occurs therefore no interfusion between the male and the female chromatine in any stage. If such a process takes place it can only be in the nuclei of the two primary blastomeres. There is reason for believing that even in these nuclei the male chromatine remains distinct from the female. It is certain that the male chromatine never unites with the female to form the primary embryonal nucleus. We cannot, therefore, maintain with Hertwig that fecundation consists in the union of two nuclei, a male and a female. No such union takes place in *Ascaris*. If it occurs in other living beings it is probably only in appearance, and it is certainly not the essential part of the phenomenon. *Fecundation is, in its essence, substitution,—i. e.*, the replacement of a part of the germinal vesicle by nuclear elements derived from the spermatozoon, and perhaps also of a part of the protoplasm of the ovum (perivitelline substance) by the spermatic protoplasm.

The elements of male and female origin do not coalesce to one segmentation-nucleus, and remain probably distinct in all the cells descended from them.

The study of the maturation of the ovum, of fecundation, and of cell-division have confirmed me in the view that the cell-nuclei are hermaphroditic, and this the more as the male chromatine does not coalesce with the female chromatine. If the male and the female pronucleus deserve their respective names, which pre-suppose their sexuality, the cell-nuclei are evidently hermaphroditic. *The tissue-cells have this character in common with the Protozoa and the Protophytes.*

I consider fecundation not as generation. This characteristic phenomenon of cell-life consists in an exchange, and not in the genesis of a new cell-individual. The substitution of certain elements of a cell by similar parts, supplied by another cell, renders possible the unlimited preservation of life, and renders further increase possible. We can conceive of generation (reproduction) only as an increase of individuals. It is probable that in reality there exists only a single mode of increase-fission. In fecundation there is no increase in the number of individuals. This

is very clear when fecundation appears in the form of conjugation, or even in the type in which it occurs in the Vorticellæ. A *Vorticella*, A, resolves itself into a macrospore, M (female gonocyt), and a microspore, *m* (male gonocyt). Another *Vorticella*, B, is similarly split up into M and *m*: M then coalesces with *m*, and M with *m*. There are formed thus two renovated individuals, A and B, but there is no increase in the number of individuals. After fecundation, as well as before, there are only two Vorticellæ. Fecundation as it is performed in the Metazoa follows the same plan. An ovum, A, is divided into a female gonocyt, G, and in polar vesicles, *g*; a spermacyt is split up into a cytophoral part, C, and a spermatozoid, *s*. We then see that G unites with *s* to form a rejuvenated cell, which is the pristine embryonic cell. Theoretically C and *g* might produce a second cell; but in reality these elements perish, so that after fecundation, in place of two cells capable of further division, there is only one. It is clear that here an increase has no more occurred than in the case of the Vorticellæ. It would seem that the power of cells to increase by division is limited; then comes a moment when they can divide no longer unless rejuvenated by the process of fecundation. In animals and in plants the only cells capable of being thus rejuvenated are the ova, and the only ones which can effect this change are the spermacyts. All other parts of the individual are doomed to death. Fecundation is the condition of the continuity of life. By it (or in it) the generator escapes death. I have taught this hypothesis since 1876, and it finds new supports in the study of *Ascaris megalocephala*.

It would be interesting to examine whether a vegetable form could be produced indefinitely by cuttings without a slow decline setting in, or without the individuals becoming enfeebled [horticultural experience points in this direction]; whether a species can maintain itself in cases where fecundation does not from time to time interrupt the series of asexual reproduction.

By the hypothesis of the hermaphroditism of cells, parthenogenesis may be explained if the formation of the polar vesicles does not take place, or if these elements after their formation reunite with the female gonocyt. In the hope of finding this conclusion confirmed I suggested to the Natural History Section of the Belgian Academy, in 1876, to propose as a prize-question the enquiry whether in parthenogenesis the ovum evolves polar vesicles. It is certain that, in most cases in which parthenogenesis has been observed, this mode

of reproduction alternates with fecundation, and it is probable that this takes place in all cases.

It is unnecessary to call attention to the bearing of these researches upon the questions of life, individuality, reproduction, and death. But we cannot help remarking that Prof. Beneden, in the words "Fecundation is, in its essence, substitution," points towards—if he does not fully establish—a connecting-link between chemical combination and organic fecundation. By so doing he confirms the great principle of continuity at a point where it has been called in question.

IV. REFORMS SUGGESTED IN THE MODE OF CONDUCTING THE CHEMICAL EXAMINATIONS, AND IN THE GIVING OF THE CHEMICAL EVIDENCE IN THE LAW COURTS, IN POISON CASES.

By Robert Galloway, M.R.I.A.

THE subject of the present paper is one of no ordinary importance, for upon the evidence of the analytical chemist engaged in suspected poison cases mainly depends the conviction or acquittal of the suspected or accused person. I entertain the opinion very strongly—knowing the possible lapses in Analytical Chemistry, even when analyses are conducted by experienced chemists—that the analyst should not be left so uncontrolled as he is at present in these very serious and difficult analytical examinations; and that he should not be allowed, in giving his evidence, to mix up medical opinions with the results he has obtained, whether affirmative or negative, in the investigation. I advocate these and other alterations of equal importance, and which I shall name presently, both in the interests of those who may hereafter be accused and in the interests of Justice; for whilst, on the one hand, no innocent person should be condemned, the guilty ought not to escape. This is no purely chemical question, interesting only to chemists and medical men; it is one which deeply concerns the public at large.

That mistakes have been made by analysts is an undoubted fact; the public generally must be even aware of it, for there has appeared not unfrequently in the public press an account of the proceedings taken in some court of law by traders who had been falsely accused of adulterating some article of food they vended, owing to the incorrect analysis of the chemist, the verdict they gained establishing that they were honest and not dishonest traders,—a matter of no small importance, although *life* or *death* did not depend upon the issue.

The analysis of milk is so comparatively simple that a properly trained analyst ought to make no mistake; yet in this article of food, as in others, mistakes have been exposed in courts of law. The means the honest trader has of clearing his character against the dishonesty imputed to him by the errors of the analyst arises from the safeguard which was introduced into the Food Adulteration Act. By one of the provisions in this Act the party procuring a sample of food from the vendor, to have it analytically examined in order to ascertain whether it is adulterated or not, must, after procuring it, inform the vendor for what object it has been procured, and the procurer must divide it into three separate samples,—one for the analyst who is employed by the local authority who has directed it to be procured; another for the trader, who may send it to be examined to any analyst he may choose to select; and the third is to be kept for submitting it to an analyst who is to act as a kind of umpire in case the trader's analyst—assuming he has employed one—and the analyst selected by the local authority do not agree in their analytical results. If it were not for this clause in the Act honest traders would seldom have an opportunity of righting themselves in the eyes of the public against the aspersions cast upon them by the analyst. This excellent provision is no doubt not always availed of by those who consider they are innocent of the charges made against them; sometimes, no doubt, owing to the poverty of the accused, at other times owing to their ignorance of the protection afforded them by the law; yet it is, nevertheless, a valuable provision, as it offers protection to the innocent, and is a restraint upon incompetent and careless analysts.

But in the still more serious charge of poisoning the accused have no such protection afforded them; in fact they have no means at all offered for defending themselves against the accusation, beyond the aid of a barrister in the law court; hence should the analyst find poison when none

had been administered, it would rarely happen that the mistake could be disproved.

That mistakes have occurred there is great reason for fearing; that iron-rust stains have been mistaken for blood stains is known: in one case, I remember well, the accused —who was charged with poisoning by phosphorus—was acquitted owing to the skilful cross-examination the analyst underwent by the counsel employed on behalf of the accused, and this was due to the fact that this lawyer had studied Chemistry. And although the notorious Palmer was no doubt guilty, it was proved more from the symptoms produced by the poison on the dying man than from any poison being found in his body. If the records of the trials for poisoning were gone through they would fully prove, I believe, the necessity for the additional precautions for protecting the innocent I advocate. The alterations I would suggest are:—

1st. There should be a medical as well as a chemical expert employed, and each separate division—England, Ireland, and Scotland—of the United Kingdom should have its own two experts, and in each case they should be specially selected by the political chief of the proper Government department. No local authority—as corporations, grand juries, &c.—ought to be allowed to select an analyst even for a preliminary enquiry. The laboratory of a general analyst is not suitable for carrying out these serious and delicate analytical examinations; nor is the commercial analyst, with his *speedy* and sometimes not very *delicate* methods, in all cases exactly in training for these more exact and refined investigations. No other analytical work ought to be conducted in the laboratory in which the poison cases are carried on; the laboratory should not be entered by an assistant, or other persons, except when the expert was present; and the expert, when leaving the laboratory, should lock it up, and keep the key in his own possession.

2nd. The chemical expert should confine himself entirely, in his examination in the court, to the question whether he had found poison or not, and the methods he employed in the examination. He should invariably bring his note-book, containing a full report of the examination, into court, so that it might be examined by all the parties concerned in the case. It is usual at present for the analyst to give, with his chemical evidence, medical opinions: this must necessarily render his evidence most confusing both to the judge and the jury, especially as, in at least ninety-nine cases out of every hundred, both the judge and the jury will

know nothing at all about Chemistry and Medicine; and it would be difficult, in most cases, to know where the chemical evidence ends and the medical begins, and how much importance is to be attached to the one branch of the evidence as compared with the other; and this is due, to no small extent, from the way the analyst generally mixes up and jumbles the two together.

Moreover, I contend that the analyst is not competent to give medical opinions, even if he has taken a medical degree. A medical man who confines himself to Surgery considers himself unfitted to act as a physician, and a physician considers himself unfitted to act as a surgeon, yet the surgeon and the physician are in practice; whereas the analyst, although he may have taken a medical degree, will not, if he practises Chemistry, practise as a medical man,—probably he has never seen any practice since taking his degree: he is therefore, I contend, incompetent to form and give medical opinions from his own knowledge and experience; they can only be the opinions he has obtained from books.

I will cite two examples as confirmatory of what I have stated. The late Professor of Chemistry in King's College, London (Wm. Allen Miller, M.D.), on being examined before a Parliamentary Committee on the water question, was asked what would be the effect on persons drinking water containing sulphate of lime? He replied that he was not competent to give an opinion on that subject. "Are you not a medical man?" inquired the Committee. "Yes," he said, "I have taken the degree of Doctor of Medicine, but I have never practised; I have therefore no experience to guide me in forming an opinion on the subject." In the Balham case the cousin (a surgeon) of the deceased man was asked in his examination some medical questions; he replied he was not able to answer them or to offer an opinion, as he was a surgeon and not a physician.

I may state that when, some years ago, I was offered by the Irish Government, through one of their legal advisers, the office of analyst in poison cases for a part of Ireland, I had to decline it from conscientious motives, although the fees I would have obtained would have added considerably to my income. I stated to the gentleman who offered me it that the evidence I would give would not conform with the evidence usually given in such cases, and therefore they might not approve of it, as I would confine myself entirely to the chemical part of the subject. I would not touch on the medical part in the slightest degree. Another reason was, I could not in my then position have a laboratory set

apart solely for that purpose, and so entirely under my own control that no one else could enter it without my knowledge and permission, which I considered absolutely necessary for conducting the analyses and examinations with that extreme care which such cases imperatively demanded.

The medical expert would of course attend and take the leading part in the *post mortem* examinations; he would also be the adviser in cases where general practitioners were in doubt as to whether a patient they were attending was being slowly poisoned or labouring under the effects of poison. In cases where the chemical analyst found poison in the body, the medical expert would, in giving his evidence, have to state whether the appearances revealed at the *post mortem* examination and the symptoms of the dying person indicated the particular poison the analyst had found. In cases where the analyst did not discover any poison, the medical expert's opinion would have to be taken as to whether the medical examinations indicated poisoning, and, if they did, what particular poison.

3rd. As the Law very properly allows those who may be charged with adulterating any article of food they sell to employ an analyst to examine a portion of the sample the local authority, by one of its agents, has obtained for the purpose of ascertaining whether it is adulterated or not, so in the still more serious matter of poisoning the suspected persons ought—if they so desired—to have the power by Law of employing an analytical chemist to be present during the examinations the chemical expert made. For if a mistake were committed by the expert, which we have shown from analogous cases is not impossible, it would in most cases be impossible for the defendant's counsel to discover it, even with the aid of an analyst, no matter how skilfully he might cross-examine the expert.

It would not, however, be desirable to allow the accused power to select any person who chose to call himself an analytical chemist for this very delicate and responsible duty; but the political chief of the Government Department which had charge of these inquiries might name some chemists of eminence, in each of the three divisions of the kingdom, he would accept.

4th. But whether in future the Government will or will not allow—and especially if they will not allow—the accused to select an analyst to be present in the expert's laboratory during the time he is conducting the examinations, no chemist, no matter how eminent he may be, should be appointed as a Government expert without having

first undergone a very strict practical examination in Analytical Chemistry, both qualitative and quantitative, and in microscopic work in relation to the detection both of organic and inorganic poisons.

One example will be sufficient to show the necessity for the candidates who are selected, or who offer themselves for the position of chemical expert, undergoing or being submitted to a practical examination such as I have indicated.

The tests for some of the organic poisons are *colour tests*, —that is, they are distinguished by the *colour* they give with certain reagents; therefore those who undertake the chemical examination for poison cases ought to be free from all *colour blindness*. Now I have found, in teaching Analytical Chemistry during many years, that a not inconsiderable number of the students I had to teach—and I have no doubt other teachers have found it to be the case with some of those they taught—were *colour blind;* they could not distinguish or see the *violet* colour potassium compounds impart to a gas flame, even when the colour was made more perceptible by cutting off, by absorption, other colour rays by viewing the flame through a glass stained *blue* with cobalt. This *violet* colour and the *yellow* colour sodium compounds impart to the gas flame were the chief ones the majority of the students had to distinguish: whether some of the students who were not defective in vision as regards the violet colour, and whether some of those who were, were colour-blind as regards other colours I am unable to say, as inorganic analytical chemistry does not afford a means of testing accurately the sight as regards other colours. But one case, a very extraordinary one, came under my notice. A gentleman, a medical man, came to study chemistry with me, and during the time he was with me he wished to go through a course of analysis for the detection of organic poisons. I found out early, after he commenced this course, that he would—on account of his defective sight as regards colours—be unable to acquire *accuracy* in this branch of chemical analysis, for he was *colour-blind* as regards most of the colours.

If it could be ascertained, I venture to say that it would be found that our Government authorities have never ascertained, before allowing an analyst to act as a chemical expert, whether he was free from colour-blindness; yet in this one particular alone my readers will see how important it is that those who are appointed to discharge the duties devolving upon a chemical expert in poison cases should be free from even this one defect.

5th. There is yet one other point deserving of notice. At the present time much of the analytical and other laboratory work is not performed by those to whom it is entrusted, but by their assistants; sometimes by students, and even laboratory porters, it is said, sometimes take part in it: nay, more, when even the brain-work as well as the manipulative skill of some of the papers contributed to our learned scientific societies—as the Royal—is not the work of the one whose name it bears, but is the work of others who do not appear *on the stage*, it is necessary, in the interests of the accused and also in the interests of Justice, that the expert should not be allowed to hand over any of the analytical examination in poison cases to another party; he ought to be examined on oath whether he had performed the entire examination himself.

From some observations our present Secretary of State for the Home Department, the Rt. Hon. Sir Wm. Vernon Harcourt, made in the House of Commons some time ago, he appears to be opposed to any alteration in the present mode of conducting poison cases; but if Sir William knew as much about Analytical Chemistry as he knows about Law, I think he would be inclined for at least some reformation in the present system.

V. VARIABLE STARS.

By J. R. Sutton.

IN the constellation of Perseus is a star which is known to astronomers as β Persei, otherwise Algol. Ordinarily this star is of the second magnitude; but if it is attentively watched it will be observed to undergo some rather remarkable changes. There are times, which from the regularity of their frequent occurrence can be readily calculated beforehand, when its light undergoes a gradual diminution, passing from the second to the fourth magnitude in about $3\frac{1}{2}$ hours, remaining at the fourth magnitude for about twenty minutes, and thence passing in another $3\frac{1}{2}$ hours to its normal brilliancy, at which it remains for some 61 hours,

the whole period occupying some 70 hours in its completion.

Several theories have been put forward to account for the strange phenomena which this star exhibits, but hitherto, as it seems, without much success. We shall examine them one by one, although some of them will not call for more than a cursory notice.

We may reject at the outset the supposition that Algol is of an irregular shape, and therefore presents, in consequence of his rotation, some parts of his surface at an ever-varying angle to the earth, the brilliancy being greatest when the greater portion of the surface is square to the line of sight. And the same reasons which enable us to reject this, enable us also to cast aside the idea that one side of Algol is brighter than the other. For it must be abundantly clear that if we suppose Algol to be in a sun-like condition, he must be surrounded—as other suns are—by an elastic gaseous envelope possessing light and heat-giving properties. It follows, then, that in the first case the tendency of these clouds would be to spread themselves over, and obliterate from sight the irregularities upon the surface of this star; whilst, in the second place, it must be regarded as next to impossible that one portion of a star's surface should be in a different condition to another, and remain so for a very long period. In all questions such as this analogy is our safest guide; and if we look upon Algol as a sun like our own, we must rather infer that the mighty forces which are necessarily in ceaseless action throughout his whole globe would tend to fully equalise the light and heat-giving power of his whole surface. Moreover, Prof. Young has indicated reasons for the conclusion that the sun's nucleus is not solid, but that, in all probability, it is in a nebulous state throughout,—a consideration which if admitted would, obviously, completely negative, upon analytical grounds, either of the above theories.

I pass, also, the sun-spot theory—the theory, namely, that sun-spots of large dimensions check the emission of light from the surfaces of the different variable stars, the observed effect being caused by the star's motion of rotation. So far as Algol alone (or indeed any other star whose maxima and minima of brightness occur with an unbroken regularity) is concerned, a theory such as this is but the re-statement, in a slightly modified form, of the theory that one side of Algol is brighter than the other. We have yet to learn that any disruption on the surface of our own sun could last unchanged for a very long period.

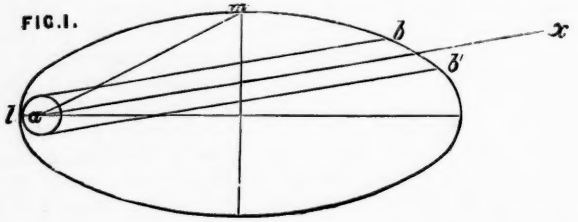
In like manner Prof. Balfour Stewart's suggested possibility must be dismissed as unfounded upon fact. "We are entitled," he says, "to conclude that, in our own system, the approach of a planet to the Sun is favourable to luminosity, and especially in that portion which is next the planet. Let us consider variable stars. Now if we suppose such a star to have a large planet revolving round it at a large distance, then, according to our hypothesis, that portion of the star which is near the planet will be more luminous than that which is more remote; and this state of things will revolve round as the planet itself revolves, presenting to a distant spectator an appearance of variation, with a period equal to that of the planet. And if we suppose a planet to have a very elliptical orbit, then for a long period of time it will be at a distance from its primary, while for a comparatively short period it will be very near. We should, therefore, expect a long period of darkness, and a comparatively short one of intense light, precisely what we have in temporary stars." This theory is, of course, possible, albeit somewhat deficient in analogous probability; but it must be borne in mind that a spectroscopic examination of one of these temporary stars has most conclusively proved that its sudden accession of brilliancy was caused by an outbreak of glowing hydrogen; and we have yet, I apprehend, to find that the periodical increase and decrease in the light of Algol is caused by a conflagration of any substance whatever. Moreover, it seems somewhat hazardous to maintain that a planet's influence could generate changes of light to such a degree, even if it could affect it at all, changes which would most obviously suggest an excessive influence over the physical condition of the whole system of which it formed a part.

Much more worthy of consideration is that theory which assumes each star to be the centre of a system of revolving worlds which in some instances pass in the course of each revolution, between their primary and the earth. We are upon safe ground here, for everything leads to the inference that other worlds do exist upon which intelligent beings may exist to utilise the metals, &c., which we know to have a place in their system as they do in ours.

Now let us, still keeping chiefly to a consideration of the star Algol, endeavour to find a theoretical satellite which will account satisfactorily for the periodic changes which its ruling sun exhibits. We will, in the first place, assume that his mass and volume are equal to the Sun's. Now Algol effects his complete cycle of changes in 69·83 hours

approximately; consequently our planet must make one complete revolution in that time. Wherefore, by our supposition as to mass and volume, a planet to revolve once in 69·83 hours must travel at a mean distance from Algol's centre of about 3,670,000 miles. But Algol undergoes a more or less diminution of light for nearly eight hours; therefore our planet, when in transit, must travel over a space equal to the diameter of Algol in that time. It follows, then, that at the time the planet is travelling with a velocity of 30 miles a second; consequently its orbit cannot be nearly circular, for so as to travel in a circular orbit at a distance of 3,670,000 miles would require a velocity of 91 miles per second, or fully three times the observed amount! Then, because its velocity when in transit falls short of the velocity necessary for a circular orbit, its orbit must be an ellipse with Algol in one of the foci. Fig. 1 represents an orbit which would do. In this figure the lines parallel to *a x* represent the direction in space of the solar

FIG. 1.



system; the planet travelling along *b b'* when in transit at a distance from Algol's centre of about 6,500,000 miles, passing at each return close to the latter's surface. And since the mean distance *a m* is 3,670,000 miles, and *a l* about 450,000 miles, the planet at its greatest distance must reach to nearly 6,900,000 miles from the ruling focus.

We see, then, that given the above data it is a comparatively simple matter to deduce a theoretical planet whose motions will accord with the fluctuations in the light of its primary. But we have omitted one element which creates a serious difficulty. It is this: the maximum light of Algol bears to its minimum the proportion of something like 5 to 1; and this is a consideration which seems to me to completely negative the satellite theory.

Let us pause to inquire into the conditions of this new feature in the problem.

Algol when at his brightest is shining with a light which call 5. At his extreme of loss of light he shines with a brilliancy which may be represented by 1. Then, *ipso facto*, he has lost four-fifths of his photometric effectiveness. Now a planet to cut off this amount of light must, supposing (as we may) that it is utterly opaque, have a diameter which would bear to the diameter of Algol the proportion of about 8 to 9. Or, by our assumption as to the mass and brilliancy of Algol,—*i.e.*, that it equals the Sun,—the planet must have a diameter of nearly 770,000 miles. These figures would, perhaps, require some modification for a thoroughly exact computation, since, in all probability, the greater portion of the light of Algol, as in the case of the Sun, comes from the centre of that portion of his disc which faces us. I have preferred, however, to leave them untouched, because the planet need not necessarily cross the star's disc centrally.

Taking these figures, then, to be correct enough for our present purposes, we arrive at some rather strange consequences:—(1) Either these two orbs are formed of different materials; or (2) one has in some way been able to retain its heat, while the other has parted with its heat in the ordinary course of nature; or (3) the density of the brighter of these is far in excess of that of the opaque body.

Now let us consider the *à priori* possibility of these instances *seriatim*. We shall, I think, as we proceed, perceive how unsafe it will be to put our confidence in a theory which is so utterly at variance with everything that analogy teaches us.

With regard to (1) it should be clearly understood that we have not the least reason to suppose that two bodies in one system should differ in their component elements. In the Solar System, from the fact that the Sun is constituted of similar materials to the Earth, we infer that the planets possess these materials also. And this view is further strengthened by the circumstance that Mars and Venus exhibit certain indications of air and water upon their surfaces, giving a yet closer sign of universal relationship in the planetary scheme. Any element such as iron or gold need not, of course, exist in the exact proportion to a planet's mass: but the disproportion, whatever it is, must be very small, so that we shall be quite within due bounds in assuming all the planets in the Solar System to be formed similarly. Most assuredly, therefore, we should not be justified, without strong positive evidence, in concluding that different laws have been in course of enactment out yonder in the star depths.

The same reasoning, of course, applies to the statement under the second head. Unless the two bodies are differently constituted, how are we to suppose that one managed to retain its internal heat long after the other had become cold and without brilliancy? All our knowledge goes to show that heated bodies of similar constituent elements—be they planets, rocks, or anything whatever—pass through their stages of cooling under a settled law and at a settled rate. We will, by way of example, illustrate this law by comparing the Earth and Moon. The Earth contains 81·5 more gravitating matter than the Moon; but its surface is greater than the moon's in the ratio of $13\frac{1}{2}$ to 1. Therefore, assuming them to have started upon their separate existences at the same time and at the same temperature, the Earth would part with its heat $81\frac{1}{2}$ times slower in virtue of its mass, but $13\frac{1}{2}$ times faster by reason of its greater extent of heat-radiating surface. By combining this proportion we shall see that the Moon would take scarcely one-sixth of the time to pass through its respective stages as would the Earth. This rule must obviously be modified somewhat when we compare bodies such as the Earth and Sun, because the greater power of the Sun would gather more external matter from the outer space to feed his decaying energies than could be possible for the Earth, so that the Sun would take, proportionately, far longer to cool than the much smaller Earth, in consequence. But Algol and its theoretical satellite are, as we have seen, so nearly of a size that they ought practically to pass through their respective stages of cooling in an equal time.

The fact, however, above stated, that bodies cool, generally speaking, under a settled law, is fatal to statement (3) also. So long as the heat of the larger orb is greater than than that of the lesser, so long will it be impossible that the mean density of the larger should be greater than the density of the smaller, *unless*—unless we re-admit some such subsidiary hypotheses as those which we have just shown to be unsound. We may, in fact, take it as a general rule that between any two orbs of equal size the density of one would be greater or less according as its internal heat was less or greater than its neighbour's.

In applying the above considerations to Algol and its theoretical planet, we shall not fail to be struck with the fact that the opaque orb must have parted with its heat at a much more rapid *rate* than the brighter, unless some accidental collision on a vast scale vapourised Algol without affecting its planet, which contingency must be regarded as

of a vastly remote possibility. If we assume, for argument's sake, that the Sun, when he has parted with his heat, will have a mean density equal to the Earth's (a very moderate estimate, since it will probably exceed it many times), its mass naturally remaining the same, we shall see that his volume will have to contract into a space occupying only one-fourth of that which he now occupies. Consequently if the satellite of Algol equals the Earth in density (as we have seen, a very low estimate considering his size), and Algol itself in density equals the Sun, we arrive at the happy result that the centre of the heat, life, and light of the system of β Persei is a secondary body in its own system,—since its mass must be much less than that of the opaque orb,—a state of things highly adapted to the philosophy of the Ptolemaists.

If it were worth while, it would be easy to deduce a circular, instead of an elliptic, orbit for the system of Algol, with mass and diameter to match. Such an inquiry, however, would be without value, since it would leave absolutely untouched the arguments urged against the third postulation. Whatever the size or shape of the orbit, or whatever the size of Algol, the opaque body would still bear the same proportion, namely as 8 is to 9, to Algol itself.

Another celebrated variable is Mira, of the constellation of Cetus. This star passes through its cycle of changes in a little over $331\frac{1}{2}$ days. Unlike Algol, which is normally at its brighest, Mira is only at its maximum of brilliancy during a period which bears a very small ratio to its period of minimum. Furthermore, its variability is not so regular as that of Algol. Its greatest brilliancy is not always of equal intensity; but, at the same time, it is supposed that the different effects occur with periodical regularity, and are only minor variations of the great cycle of changes.

Manifestly none of the above theories explain the changes of this star. Even the satellite theory, the most probable of them all, would require, it would seem, bodies whose motions should be altogether independent of the laws of gravitation. Any planet, or number of planets, to effect the observed changes, would have to rush hither and thither at times, in a highly erratic manner, to the not small danger of the whole system the while; but settling down calmly, for about five months, in such a position as to cut off nearly every particle of the light which Mira sends to the Earth.

We see, then, that every theory yet propounded is defective, insomuch that, although they may be successfully

applied in special cases, they fail utterly in reconciling themselves with the nearly similar occurrences in other stars.

There is a theory, however, by which *all* the phenomena with which I have dealt may be explained in a very simple manner. And not only has the theory I am about to propose the merit of explaining them satisfactorily, but it is founded wholly upon arguments drawn from analogy. And further, it shows also the sudden appearances of "temporary" stars, and the changes—whether regular or irregular—which the variable stars undergo, to be due to the different workings of one cause.

But it will be necessary, before proceeding, to notice briefly the appearances which some of these temporary stars have presented.

I take first the celebrated star of 1572. This star was first observed by Tycho Brahé. It seems to have flashed suddenly into existence, shining with a light which was comparable to that of Venus in quadrature; indeed for a considerable time it was visible in full sunlight at noon. It faded very rapidly, however, and finally disappeared, having been visible for very nearly seventeen months. It may be that this instance was but the reappearance of a variable star of long period; for upon previous occasions temporary stars have been observed near this spot. The evidence is of too frail a character, however, to warrant any definite opinion.

The only other instance which I shall use is that of the outburst of a star, in the year 1866, in the constellation of Corona. This appearance was noteworthy because a spectroscopic examination conclusively showed that its sudden accession of brilliancy was caused by an outbreak of hydrogen, making it, as Proctor remarks, fully 800 times brighter than it now is. Like the other, this star gradually faded, the hydrogen apparently cooling at the same time.

Both these stars were situated close to the Milky Way. We shall see presently that this circumstance is highly significant.

Now it seems to me that if we regard all the changes which take place in the light of the celestial orbs to be caused by meteors, we shall have made a decided advance upon former theories. Take first the case of Algol. If, instead of assuming that this orb is circled by a planetary orbit of the proper eccentricity, we assume that he is surrounded by extensive meteor systems, we have, certainly, a thoroughly practical and analogical way of escape from the

difficulties in the way of the former supposition. Now every star must be the ruler of more or less extensive meteor systems, although every star need not be so situated that the chiefest of these pass once in a revolution between itself and the Solar System. Algol, indeed, from his position, may fairly be deemed as occupying a position which would in a high degree enable him to collect into his system vast quantities of these tiny servitors. He is situated close by the Milky Way. Now if certain reasons which I have quite recently advanced elsewhere are valid, we must conclude that the Galactic Zone is crowded, more than any other portion of the heavens, with bodies of all sizes, from the mere atom to the mighty Sun. And it follows that the latter must subject to their sway much of the former matter. Our own Sun, though not situated within the Galactic Belt, is yet fairly off for meteoric attendants; and how many more should those stars more favourably situated possess? It is all but certain that the leading brilliants of the Milky Way are surrounded by enormous quantities.

Let us now suppose our November Meteor System to be vastly recruited, especially in its richest portion, or in "the gem of the ring" as it is termed. Then we can see that, to an observer situated far out in space upon the produced plane of the orbit of the November meteorites, the passage of the gem of the ring across the Sun's face, would cut off a considerable portion of the Sun's light—quite as effectively, in fact, as would the disproportionate planet, which the other theory requires. The figure which we have deduced for the planet's orbit would make an excellent orbit for a meteor system; it is, indeed, far more meteoric than planetary in form. It only remains to add that the total mass of a meteor system, to produce the observed diminution in the light of Algol, need only be a very small fraction of what would be necessary for a planet.

Now at first sight it might appear that, although we have here a ready explanation of the variations of Algol, we are not so fortunate when we come to apply it to the case of Mira. As we have said, this star is invisible to the naked eye during five months of the year; and it would be too much to ask us to believe that during all this time meteors are passing between this star and the Earth. Such an assumption would be almost as absurd as the satellite assumption. But we must be careful to distinguish between the varying conditions under which meteors are ruled by the central Sun. The problem presented by half a dozen meteoric orbits, for example, would be vastly different from

that presented by one or two hundred of such orbits. If the variability of Mira is indeed caused by these minute bodies, then, undoubtedly, she is surrounded by systems much more extensive than the Sun's, or those of Algol, although we may not fairly believe that she is surrounded by sufficient quantities to cut off the observed amount of her light. Now for our present purpose it matters little whether we regard Mira as a very small orb, comparatively, or as situated at a vast distance from the Earth. In either case her observed brilliancy could be explained. And in either case, if we assume that she is surrounded by a large number of dense meteor systems, we are provided with an assumption which enables us to explain even the variations of her variability.

We are almost certain that a considerable part of the Sun's heat and light is maintained by the impact of quantities of meteors upon his mass. But obviously, unless these bodies existed in large quantities, they could not do more than assist in maintaining his heat and light. For it would be necessary, before they could reach his surface, that they should, in some way, be pulled up short in their orbit, so as to be forced to fall by attraction upon the Sun's globe, since a body travelling in an orbit would never fall upon the Sun unless brought first almost or quite to rest, and there is not any way of bringing a moving body to rest save by collision with other bodies. From which we easily deduce the fact that the greater the number of meteor systems the greater the chance of collisions, and, *à priori*, the greater the quantity of matter which would eventually reach the Sun's surface, and, therefore, the stronger the emission of light. Furthermore the periodicity with which Mira goes through her minor changes could be explained as due to the fact that the density of the matter in each individual meteor ring is different in different places, precisely as in the November meteor system. In which case we can readily see that the amount of matter in her immediate vicinity and suitably placed for collision must vary accordingly.

We have, perhaps, in the rings of Saturn an instance of the approach of small bodies to the ruling centre in consequence of their private encounters. If the observations are to be trusted, these rings have been changing in detail and increasing in width ever since they were first measured. But the strongest evidence in favour of the theory I am advocating may be drawn from the changes in the brilliancy of the star η Argûs. This star is situated in the midst of a nebula, and, without doubt, vast quantities of matter must be in a continuous state of commotion therein, the effects of

which are observable in the un-uniform and erratic changes which η Argûs undergoes. It has vied in brilliancy with Sirius, although there have been times when it has been scarcely conspicuous upon the background of the sky. At the same time it is necessary to remark that opinion differs as to whether the star is really within the nebula or dissociated from it. The question will never, probably, be directly determined, although it seems difficult to examine Herschel's drawing of the nebula and then conclude that the stars in the same field are merely superposed upon it. If the star is in the nebula, my view is, of course, greatly strengthened; if it is not, the theory cannot be regarded as thereby weakened appreciably. All that need be said is that the changes of η Argûs are of better probability when ascribed to a meteoric origin than as a planetary phenomenon, and that its position would seem to supply the clue as to the source from whence its energy is derived.

Accepting the meteoric theory as true, we may divide the stars into three classes as follow:—

α. Those whose meteor systems are fixed, and not subject to any considerable changes. To this class belong the Sun and Algol.

β. Those whose meteor systems have definite orbits, but of such a richness that a considerable quantity must from time to time fall upon their sun's globe in the manner described above. To this class belong such stars as Mira and η Argûs, which possibly represent a state of things through which our own sun has long since passed.

γ. Temporary stars.

Upon this theory the phenomena of temporary stars could be considered as caused by an accidental collision in space between the matter of which the Milky Way is largely made up and the stars whose sudden appearances have from time to time astonished the world of science; or, in short, that variable stars represent the manifestations of purely local movements, whilst temporary stars point to the action of matter foreign to their systems. Moreover, the stars themselves are in motion, and it only requires that themselves and the matter with which they collide should be travelling in the right direction to ensure a collision. Nor must it be forgotten that the Solar System is liable to the same catastrophe, which, if it ever occurs, will most certainly sweep every vestige of life from the surface of the earth. But, comparing the Sun's position in space with those suns whose light reaches us from the depths of the Galaxy, and, further, taking into account the rarity of the

occurrence even among the latter, we may rest confident that the *chance* of our destruction from this cause alone is very small; so small, in fact, that we shall be quite safe in concluding that our planet will have a very long lease of existence indeed.

VI. THE LATERAL LINE OF FISHES.

By M. G. DE SÉDE.

IT is familiarly known that the name of "lateral line" has been given by ichthyologists to an organ which runs along each side of almost all fishes, extending from the head to the tail. It has been successively studied by Steno, Lorenzini, Petit, Redi, Leydig, and Schulze, the latter of whom have indicated the true path to be followed for the discovery of the functions of this line, whilst they have almost completed the investigation of its anatomy.

M. de Sède, in a thesis recently maintained before the Faculty of Sciences at Paris, and reproduced in "Cosmos les Mondes," gave an account of certain interesting experiments made for the purpose of elucidating the physiological function of this curious organ.

The fishes selected for the experiment were first submitted to the action of an anæsthetic, and then underwent the operation of re-section of the lateral nerve, which excited no reflex action due to pain. When resuscitated the subjects were left at rest in a large bowl, and some days afterwards they were placed in a vast aquarium where everything is so arranged that a fish desirous of circulating freely must make use of all its tactile resources and means of guidance. Under these conditions it was observed that the fishes which had been operated upon moved only with great caution, and were almost always the last to arrive at the distribution of food. Thus it appears that a fish able to make use of its eyes, but deprived of its lateral line, experiences a certain difficulty in finding its way.

M. G. de Sède next sought to ascertain how a fish would act if it retained the use of its lateral line, but was deprived of sight.

Two perches were blinded by removal of the eye-ball. There remained to them, then, for guidance, merely the general sensibility of the integuments and the special impressionability of the lateral apparatus in question. These organs acquire in a short time a great delicacy, for the two perches when placed in the general aquarium were soon able to guide themselves without any difficulty.

But the question now arises as to what part of this steering-power belongs to the general sensibility, and what to this lateral line? Further experiments solved this problem.

A barbel was blinded, and, by way of extra precaution, its filaments were amputated. Subsequently its lateral nerve was severed. As long as this fish—even though deprived of its eyes and beard—retained the lateral nerve it guided itself easily; but as soon as this nerve was severed it remained persistently motionless.

Lastly, a perch, blinded and deprived of its lateral line on one side only, was placed in the labyrinthine aquarium. It contrived to keep the non-mutilated side turned towards any obstacle,

These experiments leave no doubt as to the function of the lateral line.

It is a very delicate organ of touch, adapted to the requirements of an aquatic life. It is sensitive to the faintest movements of the water, takes cognisance of the slightest displacements, and gives fishes continual information on the state of the medium in which they live.

LUMINOUS PHENOMENON ROUND THE SUN.

Herr J. G. Walter, of Bremen, writes thus to the Editor of the "Naturforscher":—

"The memoir of M. Forel, in your paper of October 11th, concerning Luminous Appearances round the Sun, leads me to communicate my own observations on this phenomenon.

"After the intense evening red of last winter had ceased, and whilst nothing remarkable appeared in April and May, I remarked in June daily here, in Bremen, a yellowish brown halo round the Sun. During July, August, and September

I saw the same appearance in Switzerland, and drew to it the attention of many persons with whom I came in contact. The phenomenon was most striking about noon, in a pure blue sky, but was also distinct through gaps in the clouds, which then took an ugly leaden grey colour. On the mountains the colour was strongest, but was quite distinct in the valleys. In the last few days it is still visible, though faintly.

"As a landscape-painter I am accustomed to observe closely the shades of colour of the sky and of the clouds, and have been the more surprised at witnessing something which, to my knowledge, has not occurred for fifty years at least. As M. Forel asserts, the kind of weather—as warm or cold, wet or dry—has no connection with this phenomenon.

"I saw that the highest summer clouds, far above the peaks of the Alps, were still far beneath this phenomenon. Further, the colour of the Moon was quite unaffected, which makes me inclined to seek for the cause beyond the orbit of the Moon, and probably nearer to the body of the Sun. But before indulging in conjectures we must first know whether this luminous phenomenon has been seen all over the world, or at least in extra-European countries. It would also be desirable to know whether spectroscopic observations have thrown any light on the nature and the distance of the still mysterious matter. It is difficult to conceive that any terrestrial dust, smoke, mist, or gas can have remained so long (fifteen months) in the same position in our atmosphere."

ANALYSES OF BOOKS.

Transactions and Proceedings of the Royal Society of Victoria. Vol. XX. Issued May 30th, 1884. Melbourne: Mason, Firth, and M'Cutcheon. London: Williams and Norgate.

Among the papers here inserted in full we find two on the Influence of Light on Bacteria, the one by Dr. A. Downes and T. P. Blunt, F.C.S., and the other by Dr. James Jamieson. These memoirs form part and parcel of a controversy. The two first-named authors communicated to the Royal Society of London the results of an investigation from which they inferred that light is hostile to the development of bacteria, and probably to unprotected protoplasm in general. Dr. Jamieson objects to this conclusion, and attributes the effects observed not to light but to heat. Messrs. Downes and Blunt, in the paper here inserted, uphold their former inference, whilst Dr. Jamieson concludes, from further experiments, that the brightest diffused light is not inimical to the development of bacteria, and that full exposure to the sun's rays is not destructive to bacteria or their germs if guarded against exposure to too great heat. He admits, however, that development may be retarded.

Mr. James Stirling, F.L.S., contributes a paper on "Caves Perforating Marble Deposits," at Limestone Creek. The temperature of these caves is about 50° F., which is about the mean annual temperature of the district which lies at 3000 feet above the sea level.

Mr. T. S. Ralph discusses the occurrence of bacteria (bacilli) in living plants. Such bacilli have been previously noticed in the tissues of decaying plants, but the author has recognised them in the living cells of an apparently healthy Vallisneria, and also of *Anacharis alsinastrum*. He raises the questions: "What relation do such bacilli bear to the plant in which they are found? Are they vegetals living in commensalism with it? Are these organisms vegetal or animal in their life character? Do they await the dissolution of the cell contents in order to complete further destructive changes? Do they conduce to the zymotic changes of the chlorophyl and starch-grains occupying the cells in which they are found?"

Mr. P. H. MacGillivray, F.L.S., contributes two pages in continuation of his description of new or little-known polyzoa.

The rest of the papers, with the exception of Mr. Ellery's notes on the Rainfall Map issued by the Government of Victoria, contain little or nothing which might not have been worked out fully as well in Europe. What the world naturally expects from the colonial learned societies is research concerning local phenomena—meteorological, geological, and, above all, biological.

Exercises in Electrical and Magnetic Measurement. By R. E. Day, M.A. London: Longmans, Green, and Co.

The work before us owes its origin to the increased attention paid to the study of electricity and magnetism. The author enters upon his task with an explanation of the units now employed in electrical measurements. It may be that the names of eminent physicists have furnished the most convenient designations for some of these units. But we should not like to see the principle extended. It would sound strange if we were to hear a certain quantity of hardness in water spoken of as a "Clarke"—a standard "previous sewage contamination" described as a "megafrankland."

After this preliminary explanation Mr. Day gives in succession problems illustrative of electrostatics, of the tension balance, of statical induction, the guard-ring electrometer, electrical discharges, moments of tension and inertia, magnetism, resistance, and conductivity, branch circuits and shunts, currents in simple circuits, currents in branch circuits, the special grouping of cells, volta-meters, and electrolysis, and the chemical theory of electromotive force, thermo-electricity, battery resistance, electro-motive force, electro-magnetic measurement, magneto-electric induction, and the distribution of heat and work.

When used in connection with actual laboratory work this treatise will be found exceedingly valuable.

Longman's Magazine. No. 25, November, 1884. London: Longmans and Co.

THE article which claims our attention in this number bears the title "Honey Dew," and is a study by Mr. Grant Allen, of aphides, especially in their relation to ants. Execrable as the former insects are, there is much in their economy which is highly interesting. In no respect do the ants appear more decidedly as enemies to man than by fostering and defending plant-lice. It has occurred that when a tree has, with great labour, been freed from these vermin, the ants have introduced a fresh colony. So that to clean a rosery or an orchard it is necessary to begin by treating the ants to liberal doses of corrosive sublimate, carbolic acid, and where they have formed large nests, petroleum.

Mr. Allen ventures the surmise—by no means improbable—that the species of insects kept in a kind of domestication by ants have been "produced by the ants themselves exactly as the dog, the sheep, and the cow in their existing types have been produced by deliberate human selection." He quotes the observation of Sir J. Lubbock that the late autumnal eggs of aphides, laid on the food-plant of these insects, are fetched under cover by the ants and carefully preserved till spring, when they are taken out.

A reflection, which, if not original, deserves to be had in constant remembrance, is that "whilst we can easily exterminate large animals such as the wolf and the bear in America, or the puma and the wolverene in the settled States of America, we should be so comparatively weak against the Colorado beetle or the fourteen years locust, and so absolutely powerless against the hop-fly, the turnip-fly, and the phylloxera." One reason may be that inventors prefer devising new means for the slaughter of their fellow men, such as explosives, "aërostats," submarine boats, &c.

Among the enemies of aphides Mr. Allen enumerates the lady-birds. He overlooks the Telephori, or as they are called by children in some parts of England, soldier- and sailor-beetles. These are far more active and voracious than the lady-birds, and in some seasons even more common.

Among vegetable remedies he enumerates tobacco—which is very efficacious—quassia and aloes, but makes no mention of *Pyrethrum roseum*.

CORRESPONDENCE.

. The Editor does not hold himself responsible for statements of facts or opinions expressed in Correspondence, or in Articles bearing the signature of their respective authors.

THE SENTIENT EYE THE ONLY COLOUR BOX, AND THE BRAIN (*ENCEPHALON*) THE ONLY "*KNOWLEDGE*" BOX.

MAY I be permitted, with all due brevity, to direct attention to a contribution of Mr. Cave Thomas, with the first part of the above title in "Knowledge" of this week (12th December)? It entirely corroborates, so far as the *pseudo* external universe goes, the principles advocated in the fearless pages of the "Journal of Science" for some years back, under the terms Hylo-Idealism, Solipsism, Autopsism, Egoism, and *alia multa*. It conflicts, on the other hand, *in toto* with the theologistic article, quite on the lines of the defunct "Bridgewater," and Sir John Herschell's, treatises by Mr. Proctor, in the same number of his valuable serial, headed *Science and Natural Religion*. I quote below, with just economy of space, one or two of perhaps not the most salient sentences in Mr. Thomas's most suggestive communication:—

1st. The eye is the real seat of Light and Colour, their externality being a false misleading fallacy, founded on the dead Newtonian theory.

2nd. The light within us [he might have quoted Christ: "The light of the body is the Eye"] is not only a Bible metaphor, but a literal *scientific* truth.

3rd. That which is perceived by our *sensorium* [therefore the whole "objective" universe] is merely a property *or change of condition in our nerves*.

4th. Every fact shows Light and Colour and Sound [and all other sensations] to be innate endowments of our nature [anatomy], which merely require a stimulus for their excitation.

5th. This admitted, the whole science of Chromatics [he might have said all human Agnosis whatever] requires reconstruction,—lingering now in the sloughs of error from false hypotheses.

6th. Even a congenitally blind person has a perfect inner conception of Light and Colour, provided his optic nerves be sound.

7th. *This fact is admitted by all thinkers on the Continent, and by a few thorough scientists in England* [in our day the model Philistia].

8th. *It is a fact of which it has always appeared to me metaphysicians* [abstract thinkers] *might have made much.*

So far Mr. Cave Thomas. For very many years past I have done my best and worst to fulfil his last cogent scientific postulate, which lands us not in Theology or Theism, but in pure The-anthropism. It realises in Humanism the idea comprehended in Theism. It effectually checkmates all divine worship and ritual by showing the impossibility of transcending Manhood, and ultimately *Self*hood. All religions, natural or revealed, thus disappear, their successor being Hygiene, defined by Dr. Parkes as "supreme culture [physical] of mind and body."

ROBERT LEWINS, M.D.

Army and Navy Club, December 14, 1884.

"ARSENICAL DOMESTIC POISONING."

My attention has been called to a leading article entitled "Arsenical Domestic Poisoning," which appearcd in the "Medical Press" of November 12th.

In this article the writer refers to mine "On the Properties and Manufacture of Emerald Green," which appeared in the "Journal of Science" for August. The writer states I must have been sadly unobservant of what has been going on in the medical world as to the injurious effects of arsenical pigments in domestic fabrics. The only arsenical pigment I treated upon in my article was Emerald Green, and I stated, from *personal knowledge*, that the employed in an Emerald Green factory I was connected with for some time did not suffer from arsenical poisoning: it therefore appeare l to me a correct conclusion that, if persons who inhabit rooms the wall-paper of which is stained with Emerald Green suffer from arsenical poisoning, the effects of homœopathic doses of this substance are different from its effect in large doses.

In the reported medical cases that have come under my notice, in not one single case has the medical man eliminated all sources of error before arriving at the conclusion that his patient was suffering from arsenical poisoning. No inspection as to the discolouration of the wall-paper, no chemical examination of the air in the rooms, to ascertain whether it contained

arsenic, either in the form of arseniuretted hydrogen, or arsenious oxide, or emerald green have been made; and until these examinations—especially the last—have been made, and the results are affirmative, the medical writer must forgive me if I remain sceptical as to the poisonous effects of wall-paper when stained with Emerald Green.

ROBERT GALLOWAY.

"THUNDERBOLTS AND THUNDERSTONES."

THE term Thunderstone is not very frequently in use in the present day. The term Thunderbolt *is* very frequently used, and is applied to the lightning-flash when there is explosion or a crash. I have not heard it applied to any form of iron-pyrites, but it is a name very generally given to certain fossil remains found in gravels from the chalk, greeensand, oolite, and lias. They are of a cylindro-fusiform shape, and are the fossilised pens of cuttle-fish (Belemnitidæ). Those from the lias, on being fractured, emit a strong sulphurous smell. The material is carbonate of lime.

HENRY H. HIGGINS.

Rainhill.

NOTES.

BESTIARIAN DOINGS.—We borrow the subjoined paragraph from the "Medical News and Circular":—"It is hardly necessary for us to say that the anti-vivisectionists do not stop at equivocation or even at deliberate lying in their desire to influence public opinion. Two of them have, we are glad to say, been brought to book. One Ernst Weber has recently been sentenced to eight weeks' imprisonment for having in the journal of which he is editor published, under the heading "Vivisection of a Man," a statement that about eight years ago a Jewish physician had made a post-mortem examination of a man while only apparently dead, and therefore had performed vivisection on him. Dr. Pelz, who had performed the post-mortem in question, summoned him before a court of justice, with the above result. A clergyman of Munster, also, who was the author of the article, was sentenced to six weeks' imprisonment." The Ernst Weber here mentioned is doubtless the "knight of high orders," author of "Die Folter-Kammern der Wissenschaft," the text-book of German Bestiarians. For proofs of his former regard for truth, the reader is referred to "Wider die Humanaster," by Professor Goltz, reviewed in "Journal of Science (1883, p. 552).

M. J. C. Van Beek (*Archives Neerlandaises*) holds that the changes observed in the filtration of water or of dissolved or finely-divided matter through fibrous membranes under the influence of pressure, or of a prolongation of the experiment, are due to changes in the distance of the fibres constituting the membrane. Hence it is irrational to extend the results thus obtained to the exudation of a liquid through the coats of the blood-vessels and the capillaries.

Dr. Myers, in Surgeon-General Gordon's "Epitome of the Reports of the Medical Officers to the Chinese Imperial Maritime Customs' Services," testifies that opium-smoking does nothing like so much harm as alcoholic drinking.

"Soisom," writing in "Cosmos les Mondes" complains that "the efforts made to destroy the study of the Latin language because it is ecclesiastic destroys a bond of union among the nations which would have gone on increasing." [The efforts made to curtail classical studies have no reference whatever to the "ecclesiastical" character of the Latin tongue. That language has long ceased to be a medium of intercourse between

scientists, multitudes of whom cannot write it or even speak it with ease. We have no doubt that a memoir written in English would be more widely intelligible than if drawn up in Latin.]

M. Hureau de Villeneuve maintains that distilled water is neither disagreeable to the taste nor hard to digest, and that the absence of calcareous salts is rather an advantage than an inconvenience.

Prof. C. S. Minot (American Association) opposes the suggested trimonial system. He thinks that species should be based upon a statistical study of all known variations. [The existence of any unknown species vitiating, of course, the result.] He holds that individuals are not always homologous, the only fixed units being (1) cells and (2) the whole series of generations of cells from a single ovum,—the cell-cycle. An individual may be almost any fractional part of a cell-cycle. Roughly speaking, the higher the organism the fewer the number of individuals it comprises.

M. Duponchel ("Comptes Rendus") considers that the secular variations of the compass are explained by the preponderating action of a new ultra-Neptunian planet, Oceanus, the revolution of which is about 467 years. In 1580 and 1813 it must have passed through the longitudes 80° and 260°, and is at present in the constellation of Capricorn, about 314°.

Mr. A. C. Ranyard communicates to the Royal Astronomical Society a description of a dark-blue grey belt upon Saturn, which he observed on November 4th, 1883, and which has since been seen by other astronomers.

Mr. J. Fiske, in a work on the "Destiny of Man," contends that "the more thoroughly we comprehend that process of evolution by which things have come to be what they are, the more likely we are to feel that to deny the everlasting persistence of the spiritual element in man is to rob the whole process of its meaning." But why in man rather than in other organisms?

M. Gaudry (Academy of Sciences) describes the fossil *Eu-chirosaurus* of the Permian of Lebach in Rhenish Prussia. This reptile, besides its highly-specialised ribs, had a ventral cuirass, flexible, and made up of strong ganoid scales; its vertebræ had spinous apophyses with great lateral projections, such as are not yet known in any other animal. Hence it was able to creep rapidly on its armour-coated belly.

According to M. Mary ("Comptes Rendus,") the length of the human step increases little up to a speed of 65 steps per minute. From that point it increases decidedly up to 75 steps per minute and then decreases. The most rapid walk is at 85 steps per minute.

Professor J. S. Newberry (British Association) maintains that the phenomena of the Ice-age, when the climate and physical conditions of Greenland reached as far south as New York and Cincinnati, make it impossible to accept Lyell's hypothesis of topographical changes as the cause. The elevation of the continent was at that time less than at present.

It will be remembered that Prince Bismarck some time ago gave his medical adviser, Dr. Schweinger, the title of professor (in partibus, we presume), though not occupying any chair. The Chancellor has since, in the teeth of the medical faculty, inducted his protegé into the professorship of Dermatology in the University of Berlin. The newly-appointed official has had the audacity to challenge Professor Dubois-Raymond for refusing to recognise him. If the prince can force such a man upon the University *Academische Freiheit* is at an end.

Prof. Sir J. W. Dawson ("Geol. Mag.") maintains that the Nile, like most other great rivers, has been only in part the excavator of its own bed.

According to a writer in "Science" the lantern of the lighthouse at Cape San Antonio was made to appear of a decided red colour by a cloud of insects which surrounded it during the night of August 23, 1884. The offenders were brilliant red Hemiptera, *Dysdercus sanguinarius*.

J. Starkie Gordon ("Geol. Mag.") considers that the Eocenes of England are the muds of an æstuary which flowed opening eastwards, while an enormous river flowed from the west. England seems at that date to have been part of a continent stretching across the Atlantic.

W. F. Stanley, F.G.S. (British Association), rejects the well-known theory of Dr. Croll on the cause of former ice-periods, and quotes observations proving that the mean temperature of the Southern Hemisphere, 15·4° C., is slightly higher than that of the Northern, 15·3° C.

"Science" considers the common misuse of the term "scientific" as an incurable evil.

According to the "Cape Times" a gigantic earth-worm, from South Africa, has been sent to Mr. F. Biddard (Beddard ?), Prosector of the Zoological Society. Its length is 6 feet 5 inches, and its upper surface is of a bright green. The name of the species is *Lumbricus microchæta*.

Dr. Jorissen (Royal Academy of Belgium) finds that hydrocyanic acid, or some substance from which it is derived, is widely diffused in the vegetable world, and probably fulfils important functions.

Mr. W. H. Preece, speaking at the Philadelphia Electrical Conference, said that there had been more lying, more swindling,

and more rascality done over the question of storage batteries than over any other department of electrical science.

Mr. St. George Stock, writing in "Light," says that in the Southern Hemisphere the climate is growing perceptibly colder. "In the Banda Oriental, and in the Argentine Republic, houses used regularly to be constructed without fireplaces; now they are as regularly constructed with them."

Dr. Hammond, in a discourse delivered at Lehigh University, remarks that "Most civilised communities have enacted laws against the employment of children in severe physical labour. This is well enough. But no such fostering care does the State take of the brain of the young. There are no laws to prevent the undeveloped nervous system being overtasked and brought to disease, or even absolute destruction."

M. G. Colin ("Comptes Rendus") concludes, from a series of experiments, that—

1. Virulent agents, when introduced into animals upon which they have no injurious action, may preserve their properties untouched for one or two weeks.

2. These agents, after thus sojourning in animals where they remain sterile, may, if returned to others susceptible, produce their ordinary effects with an undiminished rapidity and intensity.

3. In certain cases these agents may produce, in so-called "refractory" animals, organic or functional diseases, severe and even fatal, though not analogous with those produced in susceptible animals.

4. Refractory animals, after having played the part of inert receptacles of virulent matter, may become the passive agents of contagion, though remaining themselves untouched.

5. The same animals may serve repeatedly for the transit of virulent matter without a former local deposit having the attenuating effects of a vaccination.

Dr. A. Mehring, at the last meeting of the German Congress of Naturalists and Physicians, read a paper on the heads of certain dogs found as mummies at Ancon, on the Peruvian coast. These "Inca dogs" represent three races—one resembling the sheep-dog, one a terrier, and another a bull-dog. Nehring considers the first-mentioned as the primitive form, while the other two have been developed. All seem originally derived from the wolf of North America, as a number of anatomic features testify. Wolves born and brought up in captivity undergo, even in the first generation, changes in the size and proportions of the skull, and in the size, form, and position of the teeth, which, when compared with the skulls and teeth of wild wolves, indicate a transition to the type of the domestic dog.

We regret to put on record the death of the eminent naturalist Dr. Alfred Brehm, on November 11th last.

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THE AGNOSTIC:

A MONTHLY JOURNAL OF LIBERAL THOUGHT.

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